FRONTIERS IN ATOMIC ENERGY RESEARCH

HEARINGS

BEFORE THE

SUBCOMMITTEE ON RESEARCH AND DEVELOPMENT

OF THE

JOINT COMMITTEE ON ATOMIC ENERGY

CONGRESS OF THE UNITED STATES

EIGHTY-SIXTH CONGRESS

SECOND SESSION

ON

FRONTIERS IN ATOMIC ENERGY RESEARCH

MARCH 22, 23, 24, AND 25, 1960

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CONTENTS

(A brief subject index will be found on p. 381)

HEARING DATES

	Page
Tuesday, March 22, 1960	1
Wednesday, March 23, 1960	83
Thursday, March 24, 1960	157
Friday, March 26, 1960	307

ATOMIC ENERGY COMMISSION WITNESSES

McCone, John A., Chairman	6
Wilson, Robert E., Commissioner	6
McDaniel, Dr. Paul, Division of Research	83
Ruark, Dr. Arthur E., Division of Research	86

STATEMENTS OF WITNESSES (BY ORGANIZATION OR AFFILIATION)

Argonne National Laboratory, Dr. Bernard I. Spinrad	158
Brookhaven National Laboratory, Bernard Manowitz	169
Carnegie Institution of Washington, Dr. Philip H. Abelson, director, geophysical laboratory	25
Department of the Interior, Harry Perry, Chief, Branch of Bituminous Coal Research, Bureau of Mines	177
General Dynamics Corp., Krafft A. Ehricke, Convair (Astronautics) Division	235
Kennecott Copper Corp., James Boyd	39
Lawrence Radiation Laboratory:	
Brown, Dr. Harold	18
Fox, Dr. Robert H	286
Johnson, Dr. Gerald W	56
Post, Dr. Richard F	98
Teller, Dr. Edward, director, Livermore branch	49
Los Alamos Scientific Laboratory:	
Grover, Dr. George M	329
Hall, Dr. David B	165
Schreiber, Dr. R. E	275
Tuck, Dr. James L	93
Ulam, Dr. Stanislaw	282
National Aeronautics and Space Administration, Dr. Robert E. English, Lewis Research Center	293
Naval Research Laboratory, Paul H. Egli, head, Crystal Branch	307
North American Aviation, Inc., J. R. Wetch, chief project engineer, SNAP reactor program, Atomics International Division	300
Oak Ridge National Laboratory, Dr. Arthur H. Snell	136
Resources for the Future, Inc., Dr. George O. G. Löf	346
Richfield Oil Co., W. J. Travers, vice president	30
Princeton University:	
Gottlieb, Dr. Melvin B	146
Smyth, Dr. H. D., chairman, university research board	11
University of Wisconsin, Dr. John A. Duffie, director, solar energy laboratory, engineering experiment station	367

MATERIAL SUBMITTED FOR THE RECORD

	Page
Additional information on irradiation of coal, submitted by Harry Perry	179
Characteristics by which nuclear reactors can perform some functions cheaper, better, or more efficiently than known alternates, table submitted by Dr. Bernard I. Spinrad	158
"Development of Solar Energy for Industrial and Domestic Uses," by Dr. George O. G. Löf	359
Formal statement of Krafft A. Ehricke, of General Dynamics, Convair (Astronautics) Division	251
Glossary	2
Joint Committee press release of March 8, 1960, announcing these hearings, together with an outline of the hearings	3
"Magnetic Mirror Machine Program," by Dr. R. F. Post	114
"Magnetohydrodynamic Power Generation Using Nuclear Fuel," by Richard J. Rosa, Avco-Everett Research Laboratory	336
"Pinch Research," by S. A. Colgate and H. P. Furth, University of California, Lawrence Radiation Laboratory	127
"Plasma Reactor Promises Direct Electric Power," article taken from Nucleonics of August 1957, written by S. A. Colgate and R. L. Aamodt	222
"Plasma Research," by W. R. Baker and W. B. Kunkel, University of California, Lawrence Radiation Laboratory	119
"Potential Applications of SNAP 8 to Space Missions," by Dr. Robert E. English	298
"Princeton University Program on Controlled Thermonuclear Reactions," by Dr. Melvin B. Gottlieb	153
"Plasma Accumulation by High Energy Injection: Present Status and Future Prospects," by Dr. Arthur H. Snell	143
Questions asked Dr. James Boyd after he testified and the answers thereto	44
"Some Special Applications for Nuclear Reactors," by Dr. David B. Hall	168
"Soviet Program for Industrial Applications of Explosions," report from Lawrence Radiation Laboratory	69
Statement of Dr. Stanislaw Ulam, Los Alamos Scientific Laboratory	285
"Summary of Plowshare Program," by Dr. Edward Teller	54
"Summary of Present Sherwood Situation at Los Alamos," by Dr. James L. Tuck	97
"The Problem of Burning the Rocks," by Dr. Alvin M. Weinberg	194
"The Prospect for Civilian Nuclear-Powered Airplanes," by A. Kalitinsky	228

FRONTIERS IN ATOMIC ENERGY RESEARCH

TUESDAY, MARCH 22, 1960

CONGRESS OF THE UNITED STATES,
SUBCOMMITTEE ON RESEARCH AND DEVELOPMENT,
JOINT COMMITTEE ON ATOMIC ENERGY,
Washington, D.C.

The subcommittee met at 2 p.m., pursuant to call, in room P–63, the Capitol, Hon. Melvin Price (chairman of the subcommittee) presiding.

Present: Representatives Price, Holifield, Aspinall, Van Zandt, Hosmer, Bates, and Westland; Senators Anderson, Pastore, Gore, and Dworshak.

Also present: James T. Ramey, executive director; David R. Toll, staff counsel, and George F. Murphy, Jr., professional staff member, Joint Committee on Atomic Energy.

Representative PRICE. The subcommittee will be in order.

Today we open a very important series of hearings. The scientists and engineers we have asked to testify and their colleagues are those we depend on as a Nation to pioneer the frontiers of atomic energy. We look to them for the vision necessary to maintain our prominence in this field.

In the nearly two decades since the energy in the atom was discovered, fates of nations and ideologies have become dependent on technological leadership in research. Preeminence in this field has become vital to us as a nation. The past record of this committee and its ability to recognize and support the work of our laboratories in the field of atomic energy is well known.

In numerous instances this committee stood alone in support of the scientists of our national laboratories and elsewhere. Our faith has been justified. We must be ever mindful that the United States and the free world are in a real struggle for leadership with the Soviets for scientific and technical supremacy. Our concern now is to maintain and increase our leadership in the future.

We look to this series of hearings for information and discussion on what our frontiers are, particularly in the field of energy applications. We want to find out what the advanced concepts are and what plans exist for carrying them through.

On behalf of the members and the staff of the committee, I want to thank the Commission, and particularly Dr. Kavanagh and his associates, for their assistance in organizing these hearings.

Before I introduce our first witness I would like to place in the record the Joint Committee press release of March 8, 1960, announcing these hearings, together with the outline of the hearings. Also a

glossary of terms which I believe will be very helpful to the layman reading this publication:

We will also include a biographical sketch of each witness with his testimony.

(The material referred to follows:)

GLOSSARY

APU—Auxiliary power unit.

ARO—Army Research Office.

Breeder reactors—Nuclear reactors that produce power and, in the same process, produce at least as much new fissionable material as is consumed.

Burnout—In controlled thermonuclear research, a term applied to the rapid reduction in the density of neutral particles in a discharge as a result of their being ionized more rapidly than they are formed.

Chemonuclear reactor—A nuclear reactor designed to utilize nuclear radiations for the production of useful chemical reactions.

Cislunar—A term describing that volume of space short of the Moon's orbital path around Earth.

Containment time—In controlled thermonuclear research, time during which the density of the plasma does not undergo substantial change.

Controlled fusion reactor—Device within which appropriate isotopes of light elements can be caused to undergo nuclear fusion, the end result being the controlled production and extraction of useful quantities of energy in excess of that required to operate the device.

Deuterium—An isotope of hydrogen having an atomic weight of 2.

Dissociation—Splitting of a molecule, as in the splitting of a hydrogen molecule ion into an atomic ion and an atom.

DT—Symbol for the deuteron-triton reaction.

Magnetic mirror—A region of space in which the magnetic field changes rapidly so as to reflect charged particles.

Magnetic pumping—One of the methods of heating a plasma by means of a periodically varied magnetic field.

Megaton—Nuclear yield equivalent to 1 million tons of TNT.

MTR—Materials testing reactor (at the National Reactor Testing Station, Idaho).

Ohmic heating—Heating of a plasma by means of a low-frequency electric discharge.

Outer solar system—The system of planetary bodies farthest from the Sun, starting beyond the radius of Mars.

Photogalvanic cell—Electrolytic cell converting sunlight to electricity.

Plasma—An electrically neutral gas of ions and electrons.

Plowshare—Name given to the AEC program for investigating and developing peaceful uses of nuclear explosives.

PWR—Pressurized water reactor.

Q—A unit of heat energy defined as equal to 10^{15} British thermal units.

Relativistic electrons—Electrons traveling fast enough so that their increase of mass (according to the special theory of relativity) is significant.

Shock heating—A method of heating a plasma by means of a sudden increase of a magnetic field.

Side product—Secondary product; with reference to fission energy, the primary product is heat and the secondary product may be a radiation induced chemical reaction.

SNAP—Systems for nuclear auxiliary power.

Fast reactor—One in which neutrons occurring in a chain reaction do not slow down before causing fission.

Flat plate collector—Blackened surface with transparent cover and fluid circulation system for conversion of solar radiation to heat.

Focusing system—Concave reflector which concentrates solar radiation for conversion to heat.

Fossil fuel—Fuel such as coal or oil that is derived from organic residues of previosuly living matter.

G—Unit of the gravitational force of the Earth.

Gnome—Name given to a proposed nuclear experiment in the plowshare program planned to take place in New Mexico.

Ignition temperature—Temperature at which a self-sustaining thermonuclear reaction can be maintained.

Impurity atom or ion—Particles, other than the fusion fuel, which tend to cool a hot ionized gas, or prevent it from reaching high temperatures.

Inner solar system—The system of planetary bodies closest in orbit to the Sun and out to and including Mars.

Ion cyclotron frequency—Frequency of revolution of an ion in a given magnetic field.

Ion, positive ion—The charged particle formed when one or more electrons are removed from an atom or molecule; often, specifically, the positively charged nucleus of a heavy hydrogen atom, capable of fusion.

Kiloton—Nuclear yield equivalent to 1,000 tons of TNT.

Lignite—A variety of coal intermediate between peat and bituminous coal, especially one in which the texture of the original wood is distinct.

Magnetic bottle—A configuration of magnetic fields for containing a plasma.

Stable confinement—As used here, the ability of a magnetic field to confine a plasma without permitting the buildup of force fields and turbulant motions which would carry particles very rapidly to the walls of the discharge chamber.

Temperature—A measure of the energy or random motion of an assembly of particles (e.g., in a gas).

Torus or toroid—As used here, a doughnut-shaped tube.

Translunar—A term describing that volume of space beyond the orbital path of the Moon around Earth.

Tritium—An isotope of hydrogen having an atomic weight of 3.

Van Allen radiation belt—A beltlike space around Earth having a high concentration of high energy primary cosmic radiation particles.

WADC—Wright Air Development Center (now Wright Air Development Division).

[From the office of the Joint Committee on Atomic Energy. No. 260, Mar. 8, 1960, A.M. papers. For immediate release]

HEARING ON FRONTIERS IN ATOMIC ENERGY RESEARCH BEGIN ON MARCH 22, 1960

A series of public hearings on "Frontiers in Atomic Energy Research" has been scheduled beginning Tuesday, March 22, 1960 and continuing to Friday, March 25, 1960, it was announced today by Congressman Melvin Price, chairman of the Subcommitee on Research and Development of the Joint Committee on Atomic Energy.

Congressman Price indicated that the hearings would begin with consideration of research in the use of nuclear explosives for peaceful purposes (known as the Plowshare project) and then proceed to the program for obtaining power from taming the H-bomb by means of controlled thermonuclear reactions. Following this the subcommittee will hear testimony on advanced developments in nuclear (fission) power reactor concepts. Adaptation of nuclear power for outer space propulsion (particularly the Rover project) and auxiliary power systems for satellites and space ships (SNAP) will then be discussed. The final session will discuss direct conversion of nuclear energy to electric power, and also consider the role of solar energy from the sun.

In commenting on the forthcoming hearings, Congressman Price said:

"Our purpose in holding these hearings is to gather together experts in these advanced fields in order to hear from them the conceptual ideas which we hope will be reduced to practical application in the late 1960's and particularly the 1970's and 1980's. Out of such a gathering as this we hope will come some creative ideas for long-range applications in the peaceful uses of nuclear energy."

The experts who will testify will be scientists who are actually carrying on or are directly acquainted with the particular research porgrams discussed, Congressman Price emphasized. Among the scientists who will be witnesses are the following: Dr. Henry D. Smyth, chairman of the University Research Board, Princeton University; Dr. Edward Teller, director of the Lawrence Radiation Laboratory, Livermore, Calif.; Dr. Kraft Ehricke, Convair, San Diego, Calif.; and Dr. W. H. Zinn, vice president, Nuclear Division, Combustion Engineering Co., Windsor, Conn. A complete list of witnesses will be issued when arrangements are completed.

Congressman Price concluded by stating as follows:

"In the past, it seems to me, we have tended to react to Soviet advances in particular fields and to counterpunch with a 'crash program' in areas where the Soviets have shown significant progress. I think this approach may be unavoidable from a short-range point of view, but for the long haul, I think it is necessary for us to establish the scope and pace of our own programs.

"We have developed a lead in atomic energy research, and I believe we have an opportunity and obligation to maintain and increase this lead by taking a long look ahead and then doing something about it."

A tentative list of subjects and dates for the series is as follows:

"Peaceful Uses of Nuclear Explosives" (Plowshare program), Tuesday, 2 p.m., March 22, 1960. Within the framework of the Plowshare program the following subjects will be discussed by the experts: The advanced research relating to the use of nuclear explosives for mineral recovery, energy production, production of isotopes, uses of industrial chemicals, petroleum production and the desalination of seawater.

"Controlled Thermonuclear Reactions" (Sherwood program), Wednesday, 10 a.m., March 23, 1960. Following a general review of progress under the Sherwood program, experts from research laboratories will discuss approaches to the control of thermonuclear energy at their particular laboratories.

"Advanced Reactor Applications," Thursday, 10 a.m., March 24, 1960. A panel of reactor experts will discuss developments in this field as they relate to future power reactor concepts, including power generation and chemical processing.

"Space Propulsion and Power," Thursday, 2 p.m., March 24, 1960. This session will cover concepts and ideas on methods of propulsion to meet the challenge of travel in outer space. Included in this discussion will be such subjects as the Rover project, the use of nuclear explosions for propulsion of space vehicles, ion propulsion and nuclear auxiliary power systems (SNAP).

"Direct Energy Conversion and Solar Energy Systems," Friday, 10 a.m., March 25, 1960. A discussion of the methods of directly converting heat energy to electrical energy, as well as a discussion on domestic and industrial uses of solar power devices.

SCHEDULE

TUESDAY, MARCH 22, 1960, 2 P.M.

	Witness
Introductory statement	John A. McCone, Chairman, Atomic Energy Commission.
General introduction to frontier hearings	Dr. Henry de W. Smyth.

PEACEFUL USES OF NUCLEAR EXPLOSIVES

(Plowshare program)

I. General introduction and advanced research applications.	Dr. Harold Brown.
II. Industrial chemicals and isotope production.	Dr. Philip Abelson.
III. Mineral recovery	Dr. N. L. Natland. Dr. James Boyd.
(*a*) Tar sands. (*b*) Oil shales. (*c*) Mining.	
IV. Summary	Dr. Edward Teller.
V. Energy production and recovery	Dr. G. W. Johnson.
Excavation. Water resources development. (*a*) Aquifers. (*b*) Dams and lakes.	

WEDNESDAY, MARCH 23, 1960, 10 A.M.

CONTROLLED THERMONUCLEAR REACTIONS

(Sherwood program)

	Witness
I. Introduction and general review of the Sherwood program.	Dr. Paul McDaniel. Dr. Arthur E. Ruark.
II. Los Alamos Sherwood research program	Dr. James L. Tuck.
III. Lawrence Radiation Laboratory	Dr. Richard F. Post.
IV. Oak Ridge National Laboratory	Dr. Arthur H. Snell.
V. Princeton University	Dr. Melvin B. Gottlieb.

THURSDAY, MARCH 24, 1960, 10 A.M.

MORNING SESSION

ADVANCED REACTOR APPLICATIONS

	Witness
I. Survey of advanced concepts	Bernard I. Spinrad.
II. General applications	David B. Hall.
III. Chemical applications	Bernard Manowitz.
IV. Coal processing	Harry Perry.
V. Seminar	Karl Cohen. David B. Hall. Bernard Manowitz. Bernard I. Spinrad. Chauncey Starr. Alvin M. Weinberg. Eugene Wigner. Walter H. Zinn. John W. Simpson.

AFTERNOON SESSION

SPACE PROPULSION AND POWER

	Witness
I. Nature of space challenge and general space propulsion concepts	Dr. Krafft A. Ehrike.
II. Rover project and nuclear explosives for propulsion	Dr. Raemer Schreiber. Dr. Stanislaw Ulam.
III. Electric (ION) propulsion	Dr. Robert H. Fox.
IV. Advanced auxiliary power systems	Dr. Robert E. English.
V. Nuclear auxiliary power systems	Mr. J. R. Wetch.

FRIDAY, MARCH 25, 1960, 10 A.M.

DIRECT ENERGY CONVERSION AND SOLAR ENERGY SYSTEMS

	Witness
I. General direct conversion concepts	Dr. Paul H. Egli.
II. Thermionic cell development	Dr. George M. Grover.
III. Development of solar power devices for industrial and domestic uses	Dr. George O. G. Löf.
IV. Solar energy applications in less developed nations	Dr. J. A. Duffie.

Representative PRICE. We are pleased to have with us Mr. McCone, Chairman of the Atomic Energy Commission.

Before we hear from Mr. McCone, the committee would like to welcome a new member of the Commission just sworn in, shortly over an hour ago, Mr. Robert E. Wilson. We are glad to have you before the committee for the first time as a Commissioner. We have had you with us on many other occasions.

Mr. McCone, we would very much appreciate receiving any remarks you may care to make to open up this important series of hearings.

STATEMENT OF JOHN A. McCONE, CHAIRMAN, ACCOMPANIED BY ROBERT E. WILSON, COMMISSIONER, ATOMIC ENERGY COMMISSION

Mr. McCONE. Mr. Chairman and members of the committee, I very much appreciate the opportunity to be present with you on the opening day of your committee's hearings on this most important subject, frontiers of atomic energy research.

This is a subject in which I am greatly interested, and I value the opportunity to participate in your discussions. President Eisenhower, in his state of the Union message to Congress, stated:

> Today we stand in the vestibule of a vast, new technological age, one that despite its capacity for human destruction has a capacity to make poverty and human misery obsolete.

The President, I am sure, was speaking of the broad spectrum of technology and all that it implies. He was speaking of the great strides and the future potentialities of chemistry and physics, of medicine and surgery, of communications and travel, and of the infinite possibilities of heretofore untouched parts of the universe.

Foremost in his mind was, I believe, the infinite possibilities offered by nuclear science and technology. Believing, as the Commission does, that the enormous potentialities of the atom as a contributor to man's advancement are only now unfolding, we must ever expand our knowledge of nuclear science.

I therefore commend your committee for the public service it is rendering by acquainting the Nation, through these hearings, with the potential benefits which the atom still holds in store for mankind.

It is a fact that the end result of research and its specific application to the solution of our current problems cannot always be accurately established in advance. In fact, it is quite to the contrary. Basic and fundamental research seldom reveal the ultimate or even the immediate applications of the end product.

Our experience tells us, however, that few advances in basic knowledge have for long gone wanting for practical application. Realizing this and recognizing the critical importance of its facilities for the conduct of research, the Atomic Energy Commission has completed and issued a study of the long-range outlook of our research laboratories.

This study was made at the request of this committee. It indicates that the avenues of research in nuclear science are manifold. Many of these avenues are time consuming. In saying this, I am speaking of years, not months. They are also expensive. Hence, this research cannot be continued on a year-to-year, stop-and-go basis.

We must establish our national objectives and pursue them energetically and with constancy. Fluctuating levels of support must not be permitted to undermine the enthusiasm and spirit of conquest in our laboratories.

Not included in the report on the role of the laboratories is the role of the individual scientist. In nuclear science, as in all other fields of research, there are a few rare individuals endowed by God with unusual and uncommon vision of the future, and a capacity and an ability to work toward the accomplishment of the goals that they foresee.

We must be constantly alert for such individuals and encourage them in every necessary way, for they serve as a powerful inspiration and a magnet thus attracting about them other outstanding scientists. I am speaking of the Einsteins, the Fermis, and the Lawrences of another day.

Just as their unusual minds opened up the horizons of the past, so the brilliant minds of today and tomorrow will unfold new and useful knowledge in the future.

We must seek out such rare men and encourage them and provide them with the tools and the materials that they require. The areas to probe are manifold. Let me comment on just a few.

High energy physics is one of the most important, active fields in contemporary science. Its pursuit is necessary to an understanding of the role of the elementary particles in the natural scheme of things, and thus to a real understanding of atomic nuclei. It has attracted to it many of the best experimental and theoretical scientists in the world. In this field, the United States leads the world.

We have had for several years now the most advanced equipment, and we are constructing new laboratories, equipping them with new and advanced accelerators. High energy physics is one of those areas of research in which continuity of support, of which I spoke earlier, is so essential.

Therefore, as a matter of national policy, I believe we must establish the level of effort required in this field and provide the support at required levels for a long enough period of time to assure the results that we seek.

Here, I again wish to stress that I am speaking in terms of years, perhaps a decade or more.

By this very fact, however, it is important to recognize the extent to which our decisions today commit in advance those who are to follow us. Hence, in this field, as well as in others, but especially in areas which necessarily involve large capital expenditures and high operating costs, we must carefully assess the future impact of our action and be certain that our decisions are thoughtfully taken.

Another difficult and long-range problem is the harnessing of the atom to produce electric power. We must not be diverted by current indications of an oversupply of oil or coal. The rising demands for energy will place a strain on conventional fuels eventually, and that makes imperative the development of economic nuclear power as an ultimate source of energy supply.

For this reason, the Commission has prepared and presented to you a 10-year program to achieve this goal. This program includes a continuing effort in fundamental research in this field. It is my opinion

that we must go further. There must be still longer range plans if we are to solve the hard problems of power from controlled thermonuclear fusion.

These problems would appear to be beyond solution in a single decade. The same would appear to be the case in developing reactors which would breed more nuclear fuel than they consume.

Somewhat closer at hand, but still in the research and development stage, is the use of nuclear energy for aerodynamic propulsion. I am speaking of nuclear-powered aircraft or the ramjet, or a nuclear rocket engine for space propulsion.

It appears to me that the United States and other countries as well, particularly the Soviet Union, have in hand much of the basic technology with respect to both the materials and the reactor concepts required for successful nuclear propulsion of aerodynamic vehicles.

We must not permit the pace of these developments to be dependent on firm requirements for specific end items, for this work, too, is of a long-range character, and our national objectives would be better served by pursuing research systematically and steadily.

Propulsion of ships by nuclear reactors offers a promising field for the application of nuclear power. The feasibility of this application has been demonstrated dramatically by the performance of the nuclear submarines. However, for commercial vessels operating competitively over the oceans of the world, economy of operation as well as dependability of performance are essential.

To achieve the degree of economy necessary does not, however, involve new concepts and new principles, but rather, a modification to meet the specific requirements of ocean commerce. Perhaps the most dramatic example of the useful application of the atom not initially foreseen is to be found in the utilization of nuclear explosives for peaceful purposes.

Despite the progress which has been made in the peaceful uses of atomic energy, such as we have been discussing, it is nonetheless true that in the minds of many throughout the world, the atom is still synonymous with the bomb. The nuclear explosive is a new and inexpensive source of energy which can be put to work to perform herculean feats now beyond man's ability or economic means.

Some of the things which preliminary studies indicate may be accomplished with the nuclear explosive are excavation, creation of harbors, canals, and the like, of equal or greater magnitude than the Panama Canal; development of natural resources by aiding in the recovery of minerals and oil; production of power, industrial chemicals and isotopes; use as a research tool for obtaining valuable scientific data in the fields of chemistry, physics, and seismology.

The Atomic Energy Commission's program for developing such uses of the nuclear explosive is called Plowshare. Through this program we hope to obtain the data necessary to convert a nuclear explosive into an energy and research tool so that the atom will then indeed be the servant of man.

Mr. Chairman, time does not permit me to discuss in detail the promise that remains to be unfolded in other areas of the peaceful atom, but I would be remiss if I did not mention my confident expectations of future and as yet unforeseen developments in the fields of biology and medicine, and agriculture and food preservation and

sterilization. Fundamental to all the peaceful applications of atomic energy is the paramount importance of assuring the health and safety of the public.

It is characteristic of most great technological advances that the benefits are coupled with some hazards and disadvantages which must be managed intelligently in order to realize the advantages. In a relatively short space of time we have learned a great deal about the biological effects of radiation. Day by day our understanding increases and concern is brought into proper perspective.

Nevertheless, we must continue with great energy our studies of the effects of radiation on man and his environment. I believe strongly that the world is entitled to know all the facts as soon as possible, and that acceptance of the atom can be based on fact and not on emotion.

In summary, it is my observation that the basic research must be pursued for its inherent promise of new knowledge. Basic developments soon find practical application which represent needed and timely advances.

Had the Thomas Edisons, the Orville Wrights, and the Alexander Bells been persuaded from pursuing their imaginative ideas because of no firm assurance of a customer for their products, the world might not yet be the beneficiary of their scientific achievements.

Thank you very much.

Representative PRICE. Senator Anderson.

Chairman ANDERSON. There was a news story in Canada yesterday afternoon about the Athabaska Sands quoting Mr. Floberg, saying that the explosion would take place 54 miles southeast of Fort McMurray. Floberg said the three-power atomic test ban negotiations at Geneva would have no effect on the Athabaska plans for a peaceful blast to take place next fall. The test was to be the most promising of all.

Following that, the Canadian Parliament had some discussion of it, and the Prime Minister was asked a question as to whether this blast would be set off without agreement with the Canadian Government.

I am sure I know what your feeling is on this and what mine is, but I think it would be well to explore that and state, if you will, whether the Commission does plan to work with the Canadian Government and whether the announcement by Mr. Floberg was properly interpreted up there.

Mr. MCCONE. Senator Anderson, as you know, we have been studying the possibility of using a nuclear device as a means of releasing the oil that is locked up in the Athabaska Sands in Alberta.

These preliminary investigations have been carried on with the several oil companies that are concerned, and through discussions with the authorities in the Province of Alberta and elsewhere in the Dominion of Canada.

Naturally, any such undertaking must be with the authority and consent and complete concurrence of the Canadian authorities. Likewise, it must be with the consent of the President of the United States who, in the final analysis, must authorize the use of the special nuclear materials.

These consents have neither been sought nor given. Until that time, no tangible or specific plans are underway for the carrying out of this

experiment. I feel it would be a very good experiment, but my feelings, and whether this should be done or not, quite naturally, are dependent upon the will and wishes of the authorities in Canada.

Chairman ANDERSON. I appreciate that because I heard you make the same statement privately, and I am sure Mr. Floberg intended that in his comments.

The Canadian press seemed to miss the point that he did believe there would be cooperation between the two Governments, and I thought it might be well if you would straighten it out.

Mr. MCCONE. When Mr. Floberg returned from his trip 2 weeks ago, he reported these conditions to the Commission, substantially as I have outlined them. I am sure that this is foremost in his mind.

Chairman ANDERSON. Thank you.

Representative PRICE. I want to commend you on the long-range research and applications of nuclear science, where you do not limit this to specific requirements, either military or civilian. I know of your interest in getting on with the nuclear propulsion program for aircraft, and I think you have made adequate reference to it in your statement. I thoroughly agree with you and am in accord with your thinking in this particular area.

Mr. Van Zandt?

Representative VAN ZANDT. I, too, would like to commend Mr. McCone for the statement. It is a statement which spells out the direction we should take in the years ahead.

Mr. McCone, what is the status of the Plowshare program?

Mr. MCCONE. I think this is to be discussed in considerable detail by witnesses who are to follow me. This I note from looking over the agenda. I think if agreeable, Mr. Van Zandt, your question will be adequately answered.

Mr. VAN ZANDT. Is the AEC actively engaged in planning?

Mr. MCCONE. Yes; the AEC is actively engaged in planning.

Mr. VAN ZANDT. Mr. McCone, would any moratorium agreed to at Geneva affect the Plowshare project?

Mr. MCCONE. There has been an agreement in principle between the Soviets and our negotiators that the use of nuclear explosions for peaceful purposes will be permitted. When I say this has been agreed to in principle, I mean just that, because the specific language of that article of the treaty has not been discussed.

Mr. VAN ZANDT. Then it must be understood that you will not use nuclear weapons in such a project as Plowshare, but a nuclear device?

Mr. MCCONE. A nuclear device; that is correct. And arrangements will be made for international observations of everything we do so that the world will benefit from anything we do.

Mr. VAN ZANDT. This may be a little beside the point, would you comment on the most recent Russian offer, that is, the weekend offer, in regard to the stoppage of atomic tests?

Mr. MCCONE. I am a little handicapped to comment on it. As you know, it was received on Saturday. I have been away from the city and I only returned this morning. I have not had the benefit of discussing the matter with my own associates in the Commission, the staff, or with others in Government.

Certainly it is something to be very carefully analyzed. We must not, however, in my opinion, expose ourselves to a long-range moratorium in areas which cannot be safeguarded.

Mr. VAN ZANDT. Have you had a chance to analyze their most recent statement?

Mr. MCCONE. Only as it appeared in the paper. I have not seen it. I would prefer not to comment on it.

Mr. VAN ZANDT. Thank you.

Representative PRICE. The chairman has a very important business session this afternoon, and he has asked to be excused later. I think unless someone has some pertinent questions, Mr. McCone, to ask you at this time, I believe you may be excused. We appreciate your appearance before the committee.

Mr. MCCONE. Thank you very much. I think the purposes of your meeting would be much better served when you hear from these eminent and capable scientists that are coming forward.

Representative PRICE. Our next witness is Dr. Henry Smyth. Dr. Smyth has always been of great assistance to the committee. He is perhaps best known as the author of the Smyth Report, the history of our wartime atomic energy program. But as you know, he has had a further distinguished career as an Atomic Energy Commissioner and head of a great university scientific organization.

I believe that by the time Dr. Smyth completes his statement you will agree that the Joint Committee believes in freedom of scientific public discussion.

We are glad to have you, Dr. Smyth, lead off the scientific presentation during this series of hearings.

STATEMENT OF H. D. SMYTH,[1] CHAIRMAN, UNIVERSITY RESEARCH BOARD, PRINCETON UNIVERSITY

Dr. SMYTH. Mr. Chairman and members of the committee, it is an honor and a privilege to be invited to make an introductory statement for this series of hearings on the frontiers in atomic energy research.

Over the past 10 years or more many of the hearings before the Joint Committee, or one of its subcommittees, have made significant contributions to the understanding of the scientific and technical problems of our time and their relation to public welfare.

I might mention as one notable example the fallout hearings of 1957, but there have been many others. Such hearings have been illuminating not only to the Members of Congress and the scientific community inside the Government and out, but also to the public at large.

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A.B. Princeton, 1918, A.M. 1920, Ph. D. 1921; Ph. D. Cambridge University, England, 1923; D. Sc., Drexel Institute, 1950; D. Sc., Case Institute of Technology, 1953.

National Research Council fellow in Cambridge, England, 1921–23; Princeton University, 1923–24; instructor in physics, Princeton University, 1924–25, assistant professor, 1925–29; associate professor, 1929–36, professor since 1936; chairman, department of physics, 1935–50; chairman, board of scientific and engineering research, Princeton University, 1954–.

Consultant on war research projects to National Research Council and to Office of Scientific Research and Development, 1940–45; consultant, Manhattan District project (atomic bombs), U.S. Engineers, 1943–45.

Member, U.S. Atomic Energy Commission, 1949–54.

Associate editor, Physical Review, 1927–30; member, editorial board, Princeton University Press, 1946–49; trustee, Associated Universities, Inc. (Brookhaven National Laboratories), 1946–49.

Fellow, American Physical Society, vice president, 1956, president, 1957; member, American Philosophical Society; fellow, American Academy of Arts and Sciences; member, Phi Beta Kappa, Sigma Xi.

Author: "Matter, Motion, and Electricity," 1939; Atomic Energy for Military Purposes (official War Department report on atomic bombs), 1945.

Generally these reviews conducted by the Joint Committee have been concerned with current knowledge and its immediate significance and application. Attempts to look into the future have been more rare and are naturally more difficult. This committee is to be highly commended for undertaking a task so essential to the future of this country.

This committee has sufficient experience to be able to distinguish between solid prediction and fanciful speculation on the part of its witnesses and to recognize that each has its value.

The joint congressional committee and the Atomic Energy Commission were set up in recognition of the sharply increasing impact of new technologies on contemporary civilization. Specifically, the discovery of nuclear fission and the development of nuclear weapons posed totally new problems in Government organization and foreign affairs. So great a change in military technology affected not only all military planning, but the whole atmosphere of international politics. At the same time, this great new source of energy, first used as a weapon of destruction, had obvious possibilities for peacetime use.

Atomic energy is an outstanding example of the growing importance of technology and the science from which technology stems. Increasingly, both the Congress and the executive branch of the Government encounter problems in which technical factors are important and sometimes predominant. The technology which results from the application of scientific knowledge is not necessarily beneficial. Technical knowledge gives men greater power to control their environment. Unfortunately, it does not automatically enhance their wisdom nor improve their morals.

The responsibility for directing into fruitful channels the power that science and technology confer upon us is shared by all of us inside the Government and out. In certain fields it is the particular responsibility of the Congress and the executive branch to give guidance.

They must fit research and development programs, supported by the Federal Government, into the context of our many other needs—political, economic, social, and educational. They must balance the value of more knowledge about cosmic rays against the value of better shoes for our infantry, or more food for the starving population of some remote land.

Your committee has a large segment of this responsibility.

The hearings beginning today are to deal with the frontiers of atomic technology. They are important in assuring that decisions are based so far as possible on solid facts and on balanced estimates of future possibilities. At best, such decisions involve many intangibles, many uncertainties.

In making such decisions, you will have to look into the future, to become prophets. You will have to evaluate the predictions that you will hear this week about probable technical advances, estimate their political or economic consequences, and make your own more wide ranging prophecies and judgments.

Prophecy takes many forms and is based on many different processes, conscious and unconscious. In its most primitive form it may simply be an instinctive conviction that what has happened will happen again. The sun will rise tomorrow because it has always risen. Such prophecies are universal, even in the animal kingdom.

The process of making scientific predictions follows a clear-cut pattern. It starts from past observations, generalizes these observations into a theory, draws conclusions from the theory which enable the prediction of future events. Sometimes the theory is left out, as in the primitive prediction that the sun will rise again. This is pure empiricism, simply the belief that what has happened will happen again, and is hardly scientific prediction in the modern sense. It can nevertheless be immensely useful.

The trouble with scientific prediction based on theory is that our ignorance is so much more vast than our knowledge, our method of calculation so inadequate to our needs. Assumptions have to be made all along the chain of reasoning. Assumptions have to be made as to the validity and significance of our observations. Hypotheses have to be made which explain the observations. Calculations based on these hypotheses often are impossible unless certain factors are deliberately disregarded. In consequence, sometimes conclusions are admittedly only approximately true or probably true. Nevertheless, competent scientists are so familiar with this process that their conclusions are often right even when their logic is incomplete. Their experience supplements their logic.

The predictions I have been talking about are specific predictions in particular fields or about a specific process. They are the kinds of predictions you may expect to hear in the next few days. They will be made by competent, honest, imaginative men, well equipped with the tools of their trade.

The most interesting predictions will involve judgments, colored inevitably by the hopes, enthusiasms, and temperaments of the men who make them. Their judgments will be disciplined by their knowledge of the facts and by their training, but will not be the automatic result of a completely logical process. Your witnesses are human beings, not computing machines. This is fortunate because computing machines have no judgment.

The hearings scheduled for this week are concerned with the production of energy and its application. The detailed program lists a variety of topics. Some deal with new techniques for recovering energy from sources long familiar, some with the development of new sources of energy, some with the application of recently developed sources of energy to old uses and new.

I would like to comment on several of these topics in a general way. I shall start by commenting on direct energy conversion and solar energy systems combined with controlled thermonuclear reactions.

In recent years it has become customary to equate high per capita consumption of energy with a high standard of living and an advanced civilization. That there is a correlation is beyond question. Whether the high per capita consumption of energy is a cause or a symptom is questionable. Or, to put it more simply, it is doubtful whether cheap electric power is more than a minor factor in the development of a backward country.

It is clear that in countries like ours the development of new sources of energy is of great ultimate importance since our increasing use of fossil fuels will exhaust the supply. Even uranium will ultimately be consumed. The only essentially inexhaustible source of energy which we know is fusion energy. We can try to release it by burning

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the deuterium in the waters of the world, or we can take it in the form of radiation from the sun, which is itself a thermonuclear powerplant of a scale and simplicity we cannot hope to match.

The two possibilities are quite different in their technological problems.

To get the fusion energy out of our supply of deuterium, we must learn how to burn it under controlled conditions. As you all know, this requires extremely high temperatures. Both the scientific and technical difficulties that face us are novel and fascinating. If they can be overcome, we still have to face the cost question.

By contrast, we already know many ways of using solar energy. The technical problems of using solar energy are really cost problems. The trouble with the energy of sunshine is that it is spread all over the place and is expensive to concentrate.

The desirability of pushing research on the use of solar energy or the energy from the fusion of terrestrial deuterium does not appear controversial. The questions for debate are questions of rate of progress and appropriate scale of support.

The same remarks apply to work on "advanced reactor concepts," the topic scheduled for the Thursday morning hearing.

In contrast, this afternoon's topic, "Peaceful Uses of Nuclear Explosions," Plowshare program, seems to me to have in it the seeds of controversy. Debate on the peaceful use of thermonuclear explosions should take into consideration many factors besides technological feasibility and costs. This topic illustrates very sharply the point I made earlier that this committee must consider technical developments in the context of political and social needs.

May I interpolate here, Mr. Chairman, a statement that I recognize that these wider aspects are not the immediate business of the hearings this week.

What I have been trying to do in my prepared statement is to set the wider stage on which, it seems to me, these hearings should take place. I do not suggest that the things I have just mentioned and will continue to discuss for a moment need to be discussed in these hearings, but I do say that the technical facts are relevant to the kinds of question I am mentioning and that that kind of question is relevant to the technical program.

I shall now resume my prepared statement.

For 15 years we have been trying to establish a worldwide system for the control and eventual elimination of nuclear weapons. As one step toward such a system, a step that is itself highly desirable even if no further steps are taken, we have been trying to get an agreement on the cessation of nuclear explosions.

Surely this committee and other committees of the Congress should consider very carefully whether the use of nuclear explosions for peaceful purposes will jeopardize our efforts at international control. Virtuous assurances that such explosions are not being used for weapon development will, I am afraid, simply not be believed. Guarantees that there are no present or future hazards to water supply will be viewed with skepticism. Since safety control will almost certainly require such explosions to be carried out by the Government, the economics of any results achieved will be hard to ascertain.

Since President Eisenhower's speech in 1953, renewed efforts have been made to use nuclear fission as a political instrument to promote peace. At one time the establishment of Euratom and the international agency appeared to be real steps in that direction. Unforeseen technical changes have greatly lessened the unifying effects of these measures, although they still may have long-range value.

Let us be very sure that our peaceful use of atomic explosions will not become a disruptive force in our international negotiations.

More than 10 years ago I said that I personally would urge that we forego all the peacetime uses of atomic energy if such self-denial would make it substantially easier to achieve international control of nuclear weapons. Even if peacetime uses of atomic energy had turned out more favorably than they have, I would still make the same statement. I now feel the same way about project Plowshare.

I am sure I shall find the presentations this afternoon interesting. Perhaps I shall be convinced that this is a cheap way to dig harbors, to release oil from shale, and salt from sea water.

I hope the witnesses will forgive me for considering these points only of peripheral importance. Even if our use of nuclear explosions for peaceful purposes promises to contribute significantly to our material welfare, I still question its political advisability in terms of international understanding and peace.

In conclusion, I turn with relief to "Space Propulsion and Power," the only one of the five scheduled topics I have not yet mentioned.

The effects of science in raising our standard of living are important and well-recognized. The effects of science in stimulating our thinking are not always so fully appreciated. To widen the horizons of the human imagination and to enhance the power of the human mind is surely one of the happiest results of the pursuit of science.

I do not know whether exploring space will bring any material benefits to men or increase the military power of nations. I am sure that the great interest aroused throughout the world by the first satellites was an imaginative response to a great human achievement.

Our own recent success in getting information back from a 95-pound sphere already more than a million miles away is a brilliant one. It is an achievement that stimulates the imagination and elevates the spirit. It would be a splendid thing if further investigations of space could be carried out by a concert of nations after the pattern of the International Geophysical Year.

Recognizing the difficulties of such a proposal, I hope that this country will press forward with its space program. Surely this is an appropriate activity for a nation owing its existence to exploration and dedicated to freedom of the mind and spirit. Such exploration of the frontiers of space will help more of us to share the feeling expressed by an 18th century astronomer when he said:

> This immense, beautiful and varied universe is a book written by the finger of omnipotence and raises the admiration of every attentive beholder.

Representative PRICE. Thank you very much, Dr. Smyth.

Are there any questions of Dr. Smyth?

Mr. Van Zandt.

Representative VAN ZANDT. Doctor, I was greatly impressed by your statement, and especially where you talked about the peaceful

use of the atom, and applying it to project Plowshare. Then also the effect the project will have on the international control of nuclear weapons.

What must we do to prepare the minds of the people of the world so they will understand the difference between the weapon and the nuclear device to be used for this peaceful project?

Dr. SMYTH. Well, Mr. Van Zandt, speaking as an educator, I think you have posed an almost impossible problem, but I will try to suggest how you might start.

In fact, Mr. McCone did suggest it. He said we should have international observers. It has been suggested that there should be complete declassification of whatever device was to be used in such an explosion. This would presumably mean publication and perhaps prepublication of details of how such a device would work.

This may help; it will help. Whether it will really succeed, I very much doubt.

This is what my principal concern is: I have a feeling, based on watching reactions for a long time, as you on the committee have, that reactions to these atomic explosions and to atomic power in general are not rational reactions.

I think the excitement caused by the French test in the Sahara is an example of the fact that these are not rational reactions. It is for this reason that I am suggesting that the simplest way to avoid trouble is to avoid using these nuclear explosions for peaceful purposes, at least for a period of years.

Representative VAN ZANDT. Would you say, Doctor, the basis of the fear the peoples of the world have of nuclear explosions is because of the contamination of the atmosphere?

Dr. SMYTH. That, of course, is true in part, and this is taken care of if you are underground. But then I think there is also the question of the association of nuclear explosions with nuclear weapons; after all, this is a separation that is very difficult to make. It requires a high degree of sophistication to distinguish between a device that you say is not a weapon but is a nuclear explosion, and a device that you say is a weapon and you can drop on somebody and blow him to pieces.

Representative VAN ZANDT. Then I take your position is that we should refrain from the employment of nuclear devices for peaceful purposes until we have an agreement at Geneva regarding the stoppage of atomic tests. Is that correct?

Dr. SMYTH. I have tried to put these opinions or suggestions of mine into the form of suggestions and questions.

Could I try to say here that it is not the business of the scientist to make these decisions. It perhaps is our business to raise the questions.

Representative VAN ZANDT. But, Doctor, your advice over a period of years has been very helpful to many members of this committee.

Dr. SMYTH. Let me put it this way: I haven't heard the testimony about Plowshare. I don't know what the advantages are. But my intuitive judgment, and such judgments have to be made in this business, would be about the way you stated, that I would hold back from this. I would not confuse the issue of stopping weapons tests by pushing the peaceful uses of nuclear explosions.

Representative VAN ZANDT. Thank you.

Representative PRICE. Are there any further questions?

Senator Pastore?

Senator PASTORE. Predicated on your last expression, Doctor, if you could achieve an agreement on international control of underground shots which do not involve contamination of the atmosphere, and are for peaceful purposes, would you urge complete cessation?

Is my question clear?

If you could reach an international agreement of international inspection of underground shots, purely and simply for peaceful development of atomic energy, would you still urge——

Dr. SMYTH. So that you still had really adequate detailed international inspection of every underground shot?

Senator PASTORE. Yes.

Dr. SMYTH. And satisfactory to everybody, to the Russians, to us, for every shot?

Senator PASTORE. It would be an international agreement, yes.

Dr. SMYTH. If you could really get that, possibly it would even promote international understanding.

Senator PASTORE. The reason I asked this question is because I am a little amazed that a scientist like yourself, and an educator like yourself, is sweeping under the carpet all of the possibilities and the potentialities of exploration of this great science, even for the benefit of mankind, because you are afraid that such peaceful exploration would arouse the suspicion of some people who would not believe us anyway.

Dr. SMYTH. I am not thinking so much of the people who don't believe us anyway.

Senator PASTORE. I mean they have no confidence in us.

No matter how good our intentions are, the Russians always seem to doubt us anyway. It seems to me that you cannot achieve any agreement with them unless it suits them 100 percent without regard to whether or not it suits anybody else on the face of the earth.

I am not debating with you now, but the point I make is this: You are saying, in essence, until we can reach a point where everyone believes the good intentions of America, we ought to stop all underground shots no matter whether they are for peaceful purposes or not.

Is that not your thesis?

Dr. SMYTH. No, I don't think that is quite it, Senator Pastore.

Senator PASTORE. Then you correct it.

Dr. SMYTH. I am saying, or what I am trying to say is, that the use of underground shots for peaceful purposes very much complicates the whole problem. If we can't get an agreement on underground shots because we can't detect them, we may decide to go on doing underground shots for military purposes. This may be all right. At least it is a clear-cut decision.

I am just saying that to complicate the problem by saying we wouldn't do underground shots for military development, but we will do underground shots that are for peaceful purposes, and try to make that distinction——

Senator PASTORE. And invite all the scientists of all the world to come here as observers and see what we are doing?

What is wrong with that?

Dr. SMYTH. I think you can make it harmless, perhaps. I hope so.

Representative HOLIFIELD. Are there any further questions?

Mr. Bates.

Representative BATES. You indicate it would be a splendid thing to have investigations of space by a concert of nations. Has there been any advocacy of that type of a program by the scientific world?

Dr. SMYTH. I know it has been suggested, Mr. Bates. I don't know of any group that is particularly pushing it.

Representative BATES. How did the Geophysical Year experiments come into being?

Dr. SMYTH. How did they come into being?

Representative BATES. Yes.

Dr. SMYTH. Through an organization called ICSU—the International Council of Scientific Unions, I believe. It is an overall international body that correlates the activities of various international unions in different fields of science.

Representative BATES. What is the position of ICSU?

Dr. SMYTH. I do not know.

Representative PRICE. Mr. Hosmer.

Representative HOSMER. I have no questions.

Representative PRICE. Thank you very much, Dr. Smyth.

The next witness will be Dr. Harold Brown, of Lawrence Radiation Laboratory, who will introduce the discussion on peaceful uses of nuclear explosion, and give us the first outline of the Plowshare program.

Dr. Brown will talk about the general portion of general research and application.

You may proceed.

STATEMENT OF HAROLD BROWN,[1] OF LAWRENCE RADIATION LABORATORY

Dr. BROWN. Let me say first, Mr. Chairman and members of the committee how happy I am to testify on Project Plowshare before the Research and Development Subcommittee of the Joint Committee on Atomic Energy.

My statement will be in the nature of an introduction. I will begin by discussing briefly the historical background of our attempts to develop industrial and scientific uses of nuclear and thermonuclear explosives. I will then outline the various general categories of industrial uses which we who are working on the Plowshare program see as having potential usefulness over the next few decades.

Finally, if there is time, I will give a bit of detail on the use of nuclear explosions as a tool for fundamental scientific investigations.

As is well known, many people are rather afraid of nuclear energy—not only of nuclear explosions, but all facets of nuclear energy. The reactor programs for power and propulsion are connected in one way

[1] Born in New York in 1927. He received the A.B. degree in 1945, the A.M. in 1946, and the Ph. D. in 1949 from Columbia University. He was a lecturer and staff member at Columbia from 1947 to 1950, and lecturer at Stephens Institute of Technology in 1949–50. In 1950 he joined the staff of the University of California Radiation Laboratory, Berkeley, and was a lecturer in physics in 1951–52.

Dr. Brown went to U.C.R.L. Livermore in 1952 as an original member of the laboratory's staff, became a division leader in physics in 1955, was appointed an Associate Director in 1958, and in 1959 was appointed Deputy Director of the Lawrence Radiation Laboratory at Livermore.

or another with the military purposes of nuclear energy. Reactors make materials used in bombs; reactor propulsion is useful for military propulsion purposes. The fact that there are inherent in all uses of atomic energy certain dangers, about which very great care must be taken, is also well known and applies to atomic energy across the board.

Because of its connection with national security the Government has a very strong influence and control over atomic-energy costs. This does, indeed, complicate the economic outlook on nuclear energy across the board. Plowshare shares all of these difficulties. We in the Plowshare program appreciate that as Professor Smyth has pointed out, and as Senator Pastore has so eloquently explained, technological applications can be for good or bad purposes. I flew here overnight in a 707. In a way, that might be described as a peaceful application of nuclear bombers. I agree that the applications have to be thought out very carefully, but I think that one must look at the technical matters first, and then I think one must wisely apply just those standards which Professor Smyth has pointed out.

You will hear later on, some information about Soviet high explosive shots in the 10-kiloton range. So far as I know, no one in the United States has questioned the purpose of these, nor has anyone suggested that they are nuclear shots. I know that no Soviet scientist has ever questioned his Government's motives in carrying out such shots, and I do not intend to do so either. I am only trying to say that this is a complicated problem, that if one does want to hide nuclear weapons development test explosions, there are easier ways open to totalitarian States to do so.

Unless carefully handled, Plowshare can also be an additional and complicating diplomatic factor. But I think in a statement from Professor Teller you will get some idea of the assurances that can satisfy the rest of the world as to the motives of the United States, if they can be satisfied as to the motives of the United States by any action of ours.

Though there was some consideration of peaceful uses for nuclear explosives even during the days of the Manhattan project and again early in the 1950's, it was only at the end of 1956 and early in 1957 that the serious and sustained effort was begun out of which grew the present Plowshare program. Two strong factors led to this reconsideration:

One factor was the invention of more sophisticated nuclear explosives, particularly those which obtain most of their energy from thermonuclear reactions. The use of thermonuclear fuel reduces both the radioactivity hazard resulting from the explosion, and the fuel cost, since these fuels are very much cheaper than fissile materials.

The other factor was the development of a theoretical understanding of the phenomenology of underground nuclear explosions, which underlies so many of the possible applications.

Since that time several underground explosions have taken place and have enabled us to learn a number of experimental facts about such processes.

In the same period, further developments have taken place in nuclear explosive design which have allowed the cost to be greatly reduced; when nuclear explosives are specifically made for industrial

uses, their light weight and small size are not a dominant feature of their design.

At the same time, this makes them much less useful, militarily.

By reducing the fraction of the energy which comes from fission, and by surrounding such explosives with a boron-containing, neutron-absorbing blanket to prevent the neutrons from the thermonuclear reactions from activating surrounding materials, one reduces the radioactivity to the point where it is very much less hazardous.

Furthermore, the explosions can be relatively confined by deep burial in the case of those made for excavation purposes, and completely contained in the case of those which exploit underground mineral resources. In this way, depending on the circumstances, all, or almost all of whatever radioactivity is produced can be prevented from spreading radiocative contamination beyond a very limited region. Nevertheless, some radioactivity will always be present, just as is the case for any application of either nuclear fission or nuclear fusion processes, be it in reactors, thermonuclear reactors, or whatever. A very great deal of care must, as in those cases, be taken to see that this radioactivity not be allowed to constitute a hazard outside a well-controlled area.

Of all the applications of nuclear explosions for peaceful uses, perhaps the most obvious, certainly the best understood, and probably the most certain of realization, is that of excavation.

We know on the basis of experience that the energy generated in an explosion will produce a large crater. Explosions can thus be used to produce harbor sites where there are no naturally occurring ones. Canals can be excavated to connect bodies of water separated by isthmuses whose relief in places can be as high as several thousand feet.

In a few special cases, the overburden might be removed from rich mineral deposits which are otherwise too deeply buried to be worked economically.

From what we now know, I think it is clear that we can do some things, such as excavation of harbors and canals, for a cost as low as one-tenth of what conventional costs would be for large projects. With future developments, it may be possible to reduce these costs another order of magnitude, to as low as a hundredth of conventional costs in some cases.

A second general industrial category is the development of mineral resources underground by means of completely contained explosions—our experience here is more limited but still substantial. We know that we can break up underground materials; we know that heat is deposited in the region around the explosion.

As a result of these facts, it seems very likely, as you will hear later today, that in the long run nuclear explosives will allow the recovery of oil from the Athabaska oil sands, by reducing the viscosity to the point where oil can be pumped. Since the amount of this oil may be as much as 600 billion barrels, this is an important prospect. It is somewhat more difficult, but one can hope to aid in the recovery of oil shales as well, if shattering makes it possible to retort the oil products from the shale in place.

An additional possibility is the breaking up of certain types of ore whose mineral content can then be leached out. One should say in general about mineral recovery that Plowshare may well enable

mankind ultimately to do open pit mining of the earth for all kinds of minerals to a very much greater depth than has been possible previously.

Possibly the most important mineral resource of all is water.

The United States has been mining its water resources at a very substantial rate. The water table in many places has been falling dangerously over the past decades. Nuclear explosions can be used to create underground reservoirs. Except in special cases, however, this is a rather expensive way of conserving water resources, even though the reservoirs can be created much more cheaply than by conventional means.

A much more generally applicable way is to connect the surface with permeable underground strata which are ordinarily separated from the surface by impermeable regions.

If we can reduce runoff and evaporation, and repressurize underground aquifers, perhaps Plowshare shall have made in that way its greatest contribution. Much more must first be learned about connecting the surface to underground aquifers. Studies already made indicate that the explosion can be arranged so that radioactivity will not be transported far enough to constitute a hazard. By that I mean in the case of the Rainier explosions, for example, only a few feet. However, in connection with water resources, radioactivity needs to be monitored most carefully, and such studies must be continued.

Containing the energy of a nuclear explosion underground so as to produce economic electrical power is perhaps the most difficult of all the proposed applications, but might be a very substantial contribution of Project Plowshare. The aim, as in Project Sherwood, is to burn thermonuclear fuel, which is cheaper than fissionable fuel and produces less radioactivity.

In Project Plowshare, however, we know how to make the reaction go, since thermonuclear explosions are now over 7 years old. What remains to be done, and it is no small task, is to solve the engineering problems concerned with containing this thermonuclear energy, keeping it at a high temperature by using either a dry medium or a large hole, and arranging for some heat transfer fluid to bring it to the surface. It may be easier to use the stored energy as process heat.

Nuclear explosives are also a cheap source of neutrons which can be used to produce isotopes for industrial applications. The major difficulty in the economic production of isotopes is that of recovery of the product from the large mass of material which is involved in the explosion. But if one can learn to use the earth as a chemical factory, as a medium in which to produce isotopes, as as a powerplant, then the benefits in cheap power, isotopes, or chemicals will have been well worth the trouble.

In the area of scientific uses of nuclear explosives there are a number of subcategories. One of the most interesting is the field of nuclear physics, and it should be noted that in this area the Los Alamos Scientific Laboratory has already made some very substantial contributions. One of their experiments used a nuclear explosion to indicate the nature of the fission resonances of uranium 235 by studying the distribution of fission products in those levels.

A large number of other neutron cross section experiments can perhaps be done better with nuclear explosions than with any other

source, since the nuclear explosion produces many more neutrons in a very short time than does any other source. As a result, neutron spectroscopy experiments can be done with very much better time resolution than by any other means. It has been estimated that the neutron fluxes obtained in a nuclear explosion of as little as 10 kilotons could be used to make measurements that would take 4,000 years using the best existing reactor and apparatus for the purpose.

A second phenomenon which has already been used for scientific purposes is the seismic signal generated by an underground nuclear explosion. By knowing the exact position and time of the source of the seismic disturbance, seismologists have been able to produce very much better travel time curves than have been produced before. These data have added a great deal to the knowledge of propagation of seismic waves through the earth, as well as to the knowledge about the inner structure of the earth, by the observation of waves diffracted at the outer boundary of the earth's core, 3,500 kilometers from the earth's center.

The Rainier and Hardtack II series of underground explosions produced a relation between amplitude and distance very much more accurate than had ever been produced from earthquake measurements, and clearly demonstrated the existence of the so-called skip zone.

Future underground explosions could be used in seismology to further clarify the structure of the crust, to compute with some certainty the depth of focus for earthquakes occurring in various regions. They might even make it possible to find out whether the inner core of the earth, within a radius of 1,500 kilometers, is solid or liquid.

The very high neutron fluxes produced in some thermonuclear reactions can be used to make isotopes not otherwise available. Studies made at Los Alamos and Livermore indicate that elements of very high atomic number, and some neutron-rich unstable lighter elements, can be obtained in larger quantities this way than would be possible by any other means. In fact, several new elements made in this way were first discovered in the debris of the Mike explosion in 1952. By studying the properties of these artificial elements, a great deal can be learned about nuclear structure.

All of these scientific applications should not be discounted merely because their current dollar value may seem small. Fundamental research of this kind could in the long run prove most valuable of all.

From what I have said, I think that perhaps you have gotten some idea of some of the possibilities involved in peaceful uses of nuclear explosions. Of course, how important Plowshare is likely to be economically depends on several factors. If it gets only marginal support, or if many of our ideas fail to work out, then it might not contribute for a long time. If our more optimistic expectations as to the resolution of the technical problems turn out to be correct, if the Plowshare program gets quite substantial support, with political circumstances allowing it to proceed at a rate governed only by technical considerations, Plowshare may well become the most important and beneficial peaceful application of the release of nuclear enregy.

Representative PRICE. Thank you, Dr. Brown.

Dr. Brown, at page 6 in your paper you referred to some of the experiments. Have you discussed the use of nuclear explosions with the National Academy of Science as a possible method of exploration for the Moho project?[1]

[1] The Mohole, Willard Bascow, cf. Scientific American, April 1959, vol. 200, No. 4, pp. 41–49.

Dr. BROWN. I believe that a talk was given about Plowshare at a meeting of the National Academy about a year ago, and at that meeting I believe Dr. Willard Bascomb was present. He is very intimately concerned with the Moho project. I think the conclusion was that it did not seem feasible to do that with what we know now. But that does not foreclose the future.

Representative PRICE. Would you give us, as far as you can, a description of the Moho project?

Dr. BROWN. What I know about it is very little. As I understand the Moho project, it is an attempt to penetrate down deep enough into the earth's crust—and I believe they start off in the ocean somewhere because the crust comes nearest to the surface there—so as to try to find out what effects can be found and what measurements made on the discontinuity which is believed to exist in the earth's crust at that depth.

The material of the earth's crust at that point is supposed to change its physical composition under the intense pressure from above.

Representative PRICE. At page 6 you talk about the seismic "skip" zone. Would you tell us what that is?

Dr. BROWN. I have become an amateur seismologist by force of events, having spent a fair amount of time in Geneva arguing about seismology with other people who did not know much about it either. The situation is this:

A source near the earth's surface propagates down until it comes to a region of high velocity. Then it propagates along that surface and then comes up again. What is found is that between about 1,000 kilometers and about 2,500 kilometers the waves do not come up directly; that is, they behave as if they weren't rays at all. They just kind of spread out. It is impossible to tell when the first arrival of the wave is between those distances.

This, for that reason, is called a skip zone. It is only a name to indicate that less is known about what happens to the waves in those regions and they give a smaller signal in those regions.

Representative PRICE. On page 3 you talk about the creation of underground reservoirs through nuclear explosion. Does this present a problem of contamination of the water table?

Dr. BROWN. It does, unless it is handled very carefully. I think we don't yet know enough to say for sure just what the best way to do this is. We know from the Rainier explosion that the radioactivity was rather closely confined to a region of about 55 feet in radius for that particular explosion. Yet the rock above was broken up to a distance of 400 feet and no solid radioactivity was present in that region. As a result, one could run water through that region made permeable by the collapse of the rock above without introducing the water into the region of principal radioactivity.

Another thing that was found out by very careful measurements on the ground water that does flow near the Rainier site, most of it artificially introduced pumping water, that is, we pump water down and see what happens to the radioactivity, we find for that particular constitution of material, those particular minerals, and this is so for many other minerals as well, that the minerals retain their radioactivity extremely well.

A glass was formed in the Rainier explosion which is rather insoluble in water. Furthermore, there is a constant pickup and re-

deposit of radioactivity, as happens in many chemicals in exchange columns. As a result, there is a very slow diffusion of radioactivity. The water, even if it is moving through this region, and we would try to arrange it so that no water was moving through the radioactivity region, picks up the radioactivity and then deposits it and then picks it up, with the result that after a year the radioactivity has moved only a few feet and is expected to move approximately only as the square root of the time.

Representative HOSMER. However, many materials remaining soluble and having radioactivity induced into them will become dissolved in the water and carried on in the normal fashion; is that right?

Dr. BROWN. They will become dissolved in the water. For this reason, I think it desirable to pick a mineral formation which has the least solubility. But even where there is solubility, the radioactivity gets dissolved but then the water moves and redeposits the radioactivity in a short distance, so there is a constant pickup and redeposit, which makes the movement of the radioactivity very much slower than the movement of the water.

The only way that the radioactivity would move rapidly with the water is if it were suspended in the water. If it is dissolved, it tends to reprecipitate out as soon as you move into a slightly different region.

Representative HOSMER. Over a period of time, your explanation is that it spreads through a larger area?

Dr. BROWN. Well, it spreads through a slightly larger area, but, as I say, after a year what we have measured is that it has only moved 10 feet, and after 10 years it should go 3 times that far, and after 100 years 10 times that far. Of course, radioactivity never wholly goes away.

Representative PRICE. Mr. Bates.

Representative BATES. Doctor, how do you recover your isotopes from your ground explosion?

Dr. BROWN. This is a rather difficult problem. It depends on whether we are able to contain the explosion in a large hole, in which case what you can try to do is to get some fluid in which will pump the isotopes up, or whether you are forced to do it in such a way that the material around the explosion melts and mixes. In that case, you would have to mine it out and it would be very much more expensive to do.

Representative BATES. On these seismic waves, do any of those ever go through the center of the earth?

Dr. BROWN. The core seems to be opaque to waves, but it is not understood too well.

There does seem to be a shadow on the other side of the earth from the core. Part of this has to do, I believe, with the question of whether the core is liquid or solid.

For example, if the very inner core is liquid, then it will not sustain shear waves at all.

Representative BATES. Do you ever get signals from a place on the surface of the earth which is farther away from the source or the origin of the explosion than a place which is closer?

Dr. BROWN. The skip zone does do something like this. That is to say that you get a stronger signal at 2,700 kilometers, for example, than one at 2,000.

I am not sure I understood the question.

Representative BATES. I think you have answered it.

Representative PRICE. Mr. Ramey.

Mr. RAMEY. In relation to the hazard of radiation as applied to water in underground nuclear tests, would it be possible to set up a series of experiments to explore this without creating a great hazard to anyone?

Dr. BROWN. Yes. I think you would start off by doing small experiments. In fact, you would start off, I think, by doing some more high explosive experiments and then have low yield nuclear experiments.

It is even true that you can do experiments telling how far does radioactivity diffuse in a soil of a certain kind with water running through it at a certain rate in the laboratory.

Mr. RAMEY. But you might need a nuclear explosion or two to really find out?

Dr. BROWN. I think the difference is that between a laboratory experiment and field experiment in which you are faced with all the problems that you have not thought of as well as the ones you have. Only the latter is fully meaningful, because in the laboratory you can control what the problems are.

Mr. RAMEY. Thank you.

Representative PRICE. Thank you very much, Dr. Brown.

The next witness will be Dr. Philip H. Abelson, of Carnegie Institution.

STATEMENT OF PHILIP H. ABELSON,[1] GEOPHYSICAL LABORATORY, CARNEGIE INSTITUTION OF WASHINGTON

Dr. ABELSON. Mr. Chairman, it is an honor to be asked to make a presentation here. However, my task today is a rather unenviable one. I must attempt to gaze into a very cloudy crystal ball and envision clearly the problematic future of 10 to 20 years hence. Unfor-

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[1] Born, Tacoma, Wash., Apr. 27, 1913. Married, 1936; 1 child.

Education: B.S. (chemistry), Washington State College, 1933; M.S. (physics), Washington State College, 1935; Ph. D. (nuclear physics under Prof. E. O. Lawrence), University of California, Berkeley, 1939.

Research assistant, Physics Department, University of California, Berkeley (identification of uranium fission products), August 1938–June 1939; assistant physicist, Department of Terrestrial Magnetism, Carnegie Institution of Washington (codiscovery of neptunium), September 1939–June 1941; Naval Research Laboratory, associate physicist, 1941–42; physicist 1942–44; senior physicist 1944–45; principal physicist 1945–46; civilian in charge, NRL Branch, Navy Yard, Philadelphia, 1944–45 (design of liquid thermal diffusion plant for separation of uranium isotopes); staff member, Department of Terrestrial Magnetism, Carnegie Institution of Washington (investigations of biosynthesis in micro-organisms), August 1946–September 1953; Director, Geophysical Laboratory Carnegie Institution of Washington (investigations of organic constituents of fossils and sedimentary rocks), September 1953 to present time.

Chairman, Committee on Radiation Cataracts, National Research Council, 1949–57; member, Subcommittee on Shock, National Research Council, 1950–53; member, Advisory Board, Journal National Cancer Institute, 1947–52; member, National Institutes of Health Biophysics and Biophysical Chemistry Study Section, 1956–59; editor, Researches in Geochemistry, John Wiley & Sons, New York, 1959; coeditor, Journal of Geophysical Research since 1959; member, National Institutes of Health Physical Biology Training Grants Committee since 1958; member, AEC Plowshare Advisory Committee since 1959.

Membership in learned societies: American Physical Society; American Chemical Society; Society of American Bacteriologists; Mineralogical Society of America; Geological Society of America; Seismological Society of America; American Geophysical Union; American Association of Petroleum Geologists; British Biochemical Society; the Geochemical Society; Washington Academy of Sciences; Geological Society of Washington; Philosophical Society of Washington; American Academy of Arts and Sciences; Biophysical Society; elected to membership in National Academy of Sciences, April 1959.

Phi Beta Kappa; Sigma Xi.

Awarded Navy Distinguished Civilian Service Medal, 1945; annual award physical sciences, Washington Academy of Sciences, 1950.

Areas of research and specialized knowledge: Nuclear physics; radiochemistry; biochemistry; physiology; microbiology; geochemistry, paleobiochemistry.

tunately, data concerning my topic—chemical phenomena attending underground nuclear events—is indeed fragmentary.

Our present knowledge is based almost entirely on studies of the events in water-soaked volcanic ash in Nevada. It would be difficult to choose a medium less suitable for providing information relevant to the chemical possibilities of underground nuclear detonations.

First, the overall composition is uninteresting. Second, experiments with a soft, porous material are almost useless as a base for estimating events in hard, solid rocks. Furthermore, the presence of enormous quantities of water doubtless distorted the thermal history since much heat was used in volatilizing water. Developments after the events were further affected when the roof fell in. This is to be expected when incompetent rock such as volcanic ash is involved, but might not occur in another type of rock.

The Rainier shot and others which were fired in October 1958 were, of course, designed with other considerations predominating. The Rainier event principally revealed that large quantities of rock can be heated to very high temperatures, and nuclear detonations contained underground.

The Tamalpais event of October 1958 was more productive of information. The device employed was surrounded in part by wood and paraffin. Subsequent examination of the residual gaseous medium in the vicinity of the zero point disclosed large amounts of hydrogen and carbon monoxide. It was obvious that the nuclear detonation had been accompanied by large-scale chemical reactions. That is, tons of woods and paraffin vanished; corresponding amounts of hydrogen and carbon monoxide appeared.

Two features of nuclear devices are of particular interest. The first is the enormous energy which can be released—a megaton detonation releases energy equivalent to one thousand trillion calories, which is the amount of energy produced when 150,000 tons of coal are burned.

Costs of nuclear devices are not readily available, but a reasonable guess is that energy might be available at an expenditure equivalent to less than $1 for a ton of coal.

I might say parenthetically that the potential amount of energy that is available in the light elements is such as to supply man's needs for between 100 million and 1 billion years.

A second feature of nuclear explosions is the enormous temperatures which are reached—conditions attainable only with great difficulty by other means.

These features provide a basis for speculation on certain important potentialities of underground nuclear detonations. By considering some of the phenomena known to accompany nuclear explosions, an estimate can be made of likely events and chemical reactions.

Let us first consider the general sequence following detonation of a 10 kiloton equivalent device underground. During the first thousandth of a second the nuclear energy (10^{13} calories) is largely converted into other forms such as radiation, shock, and chemical energy. At about this stage a weight of approximately 2,000 tons of material is at about 14,000° Fahrenheit, at which temperature all chemical compounds are unstable. Even the most refractory substances are converted into atomic gases.

Thus, the detonation of the nuclear device can be looked on as a means of producing unprecedented quantities of matter in the form of free atoms. The temperature of this mass of atoms, however, quickly diminishes as more of the surroundings are engulfed, melted, or volatilized and energy is dissipated in the form of shock waves and mechanical work.

In the neighborhood of temperatures of 10,000° Fahrenheit simple compounds which do not ordinarily exist at ordinary temperatures, such as SiO, Al_2O, are stable and these are quickly formed. At progressively lower temperatures other chemical combinations occur. Depending on the composition of the rock, the new compounds formed may be quite different from those originally present.

To illustrate more specifically the kinds of events which may occur, let us consider some examples—first of all, a nuclear detonation in limestone which for our purposes we will consider to be $CaCO_3$. For simplicity, the chemicals contained in the device are not considered.

If a 10-kiloton device were detonated about 2,000 tons of $CaCO_3$ would be immediately converted into elemental calcium, carbon, and oxygen. As the hot mass cooled, carbon monoxide would form, then calcium oxide (lime).

The calcium oxide would exist first in the form of a gas. With further cooling at about 5,160° Fahrenheit, it would liquify, becoming a solid at 4,676° Fahrenheit. Carbon dioxide would form in increasing amounts at lower temperatures. However, the calcium oxide and carbon dioxide would not begin to combine until temperatures below 2,200° Fahrenheit were reached.

The time required for the mass to cool to 2,200° Fahrenheit is difficult to estimate. However, one can visualize the possibility that the gaseous carbon dioxide could be conducted away from the detonation site before it reacted, thus leaving CaO or lime as a product. Allowing for some heat losses, approximately 10,000 tons of original limestone would be in the form of CaO and CO_2.

The price of lime at the kiln is in the neighborhood of $10 per ton. A very crude estimate based on use of megaton devices indicates that the cost of production in the ground via nuclear devices might be about $1. I do not wish to advocate this method as a new and wonderful way of producing lime; rather, I cite this example to show that underground nuclear costs are not impossibly high.

Among other problems there would be the mining of the lime which would probably be more expensive than the nuclear device used to create it. The lime has the interesting property of possessing potential chemical energy, for on adding water much heat is set free. Thus, a substantial fraction of the original nuclear energy would be converted into controllable potential chemical energy and might be tapped gradually.

This is one of the very important potentialities that we must look for in the trapping of nuclear energy. We must look for means whereby this nuclear energy, which is suddenly released, can be converted into useful chemical energy, which can be tapped at leisure.

Let us consider a second example, one which may be of greater economic importance, namely, a detonation in carbonaceous shale. For simplicity, assume a composition of 20 percent carbon and 80 percent clay. Immediately after detonation, elemental Al, Si, C, H,

and O would be present. On cooling to 3,000° Fahrenheit, the products would include carbon monoxide CO, H_2, and SiC. It might be possible to devise means of conducting the gaseous CO and H_2 from the reaction scene leaving solid SiC (carborundum). Later water could be added to the hot SiC yielding methane (natural gas).

Another and probably more feasible possibility is to include sufficient water at the original detonation site to be chemically equivalent to the carbon present, that is:

$$C+H_2O \rightarrow CO+H_2$$

This is the so-called water gas reaction, and the mixture would constitute a fuel as well as a source of raw materials for further chemical synthesis.

Water gas has been sold commercially for many years. The value of this material is 1 to 5 cents per 1,000 cubic feet. Only the crudest nuclear cost estimates can be made. These are in the neighborhood of 2 cents per 1,000 cubic feet, assuming use of megaton devices.

Alternative methods of utilizing carbon monoxide lead toward a whole series of products. Consider the reactions:

$$CO+H_2O \leftrightharpoons CO_2+H_2$$

and

$$4CO+2H_2O \rightleftarrows CH_4+3CO_2$$

In both cases the extent of the reaction is strongly dependent on concentration of CO_2. A cheap way of removing CO_2 could lead to either hydrogen gas, synthesis mixture suitable for Fischer-Tropsch production of hydrocarbons, or methane.

In our first example—decomposition of $CaCO_3$—we saw how enormous quantities of lime could be produced underground. This could be used as an absorbent of CO_2. In many areas limestone beds are found above or below carbonaceous sediments.

Another scheme has been proposed by Dr. John Grebe of Dow Chemical Co. He suggests detonation of a device in a limestone rich in organic matter. A simplified chemical equation of events that might occur is

$$CaCO_3+4C \rightarrow CaC_2+3CO$$

Again it would be necessary to remove CO from the reaction scene. The carbon monoxide might be used in one of the ways indicated above. The other product CaC_2 is calcium carbide. Addition of water to this substance releases acetylene—a very important and valuable starting material for many chemical processes.

In this presentation I have emphasized chemical reactions involving fossil organic substances. The amounts of these present in sedimentary rocks is enormous. Only a trivial fraction, perhaps one-thousandth—is present in concentrations or environments suitable for commercial exploitation employing present conventional methods. Successful development of schemes based on nuclear detonations could increase the available fuels or petroleum by more than 100 times.

A further series of chemical products of underground nuclear detonations could be imagined. Enormous holes deep in the earth might

be used as gigantic natural pressure chambers into which the chemical reactants were introduced and caused to react by nuclear means. Further speculation, however, seems inappropriate in view of our present virtual total ignorance of the actual course which nuclear detonations might follow in various rock environments.

Our present lack of knowledge could be in good measure remedied by a systematic program utilizing 5 to 10 devices of 1 to 10 kiloton equivalent in a variety of underground media, including limestone, carbonaceous shale, granite, and gabbro.

Out of such tests would come basic data needed to design larger scale events and to give a much better basis for engineering estimates of costs and commercial potentialities. Perhaps more important, the new knowledge might point toward quite different and much more sophisticated means of utilizing nuclear energy.

Efficient conversion of violent thermonuclear energy into more tractable, controllable forms, such as useful chemical energy, will surely one day be attained. A few imaginatively conceived experiments might well point the way to this achievement.

Representative PRICE. Are there any questions?

Representative VAN ZANDT. I have a question, Mr. Chairman.

When you refer to using a device in regard to your uncovering coal, you say the costs of nuclear energy are not available, but a reasonable guess is that energy might be available with an expenditure equivalent to less than $1 per ton of coal.

Would you explain what you mean there?

Dr. ABELSON. When you start attempting to employ a nuclear device, there are all kinds of costs that go into the application. Of course, first, there is the device itself, and, secondly, is the mining operation necessary to place the device. It is difficult, without experience, to know what these things are going to cost.

Furthermore, the AEC has chosen to release certain cost estimates for devices, but one knows from all past experience that these costs tend to decrease as technology improves. Any estimate we might make today, no matter how accurate would be outmoded in a short time. Hence, it is not really useful to be too precise. It is better to give just approximate estimates.

Representative VAN ZANDT. Thank you.

Representative PRICE. Mr. Bates?

Representative BATES. I wondered, as I listened to the previous question, that it sounded pretty good coming down the road, at $10 to $1, and then you indicated that you do not advocate it.

What is there of a concrete nature which shows great promise for this direction?

Dr. ABELSON. I am trying to hedge in this presentation. I believe in the great potential of Plowshare, but I think it is just plain stupid to try to engineer applications before one knows what the facts are. We just do not have enough facts at our disposal to really make sensible estimates. We kid ourselves when we try to make numbers.

Representative BATES. So at this point it is good for talking and thinking about it, but as far as taking any concrete action, we should wait?

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Dr. ABELSON. What I was trying to do was to indicate that the potentialities are very good, that they are something to be serious about. On the other hand, I wanted to stay away from trying to give numbers which might be considered to be more reliable than they really are.

Representative BATES. Look at it, but don't buy it right now?

Dr. ABELSON. I would advocate that within the framework of the disarmament talks at Geneva, that at the earliest possible moment we get the information necessary to really give an evaluation of the chemical potentialities of underground nuclear devices.

Representative PRICE. Are there any further questions?

If not, thank you very much, Dr. Abelson, for a fine paper.

The next witness will be Mr. William J. Travers, vice president of the Richfield Oil Corp.

Mr. Travers will talk on mineral recovery, specifically on the subject of tar sands.

STATEMENT OF W. J. TRAVERS,[1] VICE PRESIDENT, RICHFIELD OIL CORP.

Mr. TRAVERS. Mr. Chairman and members of the committee, first I would like to say that we appreciate the privilege of presenting to this subcommittee Project Oilsand, which is a proposal by Richfield Oil Corp. of Los Angeles, Calif., and its associates, Cities Service Co. and Imperial Oil, Ltd., to use nuclear energy in an experimental effort to recover oil from the deeply buried oil sands in the Athabaska area of the Province of Alberta, Canada.

The general location of the deposit which we are discussing is shown on the map hanging on the easel.

Richfield has been working on this project in close cooperation with scientists at the University of California Lawrence Radiation Laboratory.

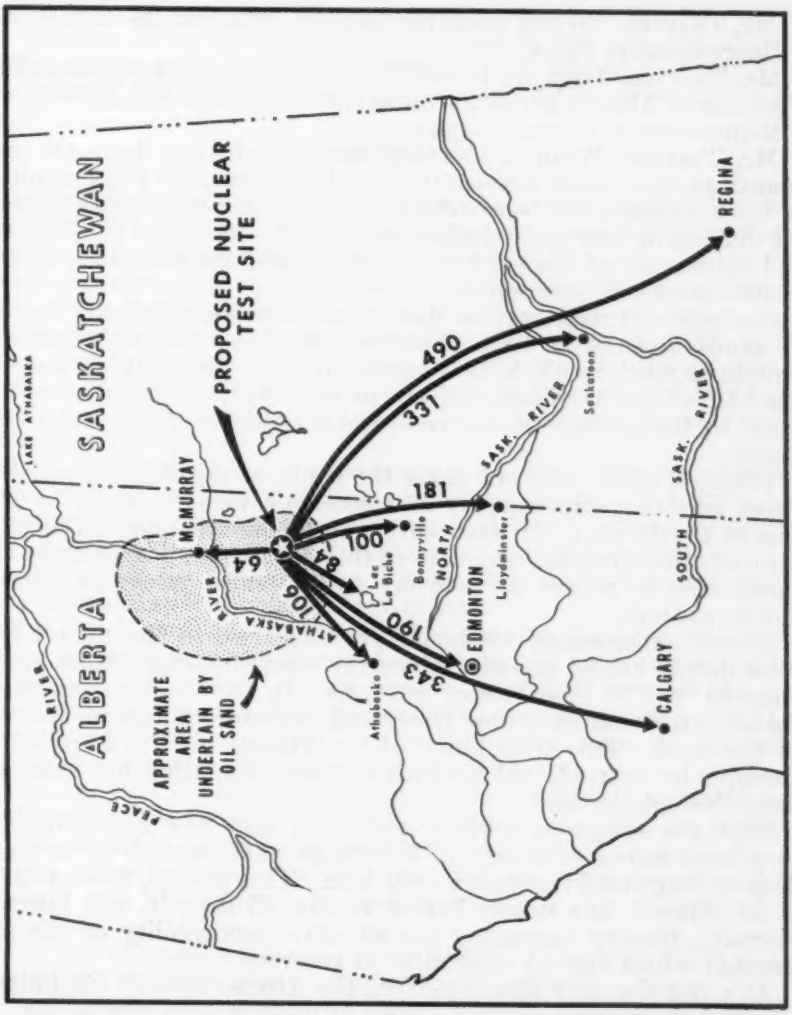
Although commonly referred to as tar sands, the Athabaska sands are more properly called oil sands because they are not filled with the usual tarry residual oil from which the light gas-oil fractions have been evaporated but, instead, are filled with what the late Max Ball called a young oil containing a full range of petroleum fractions.

Representative PRICE. Would you describe the map that you have there?

(The map referred to appears on p. 31.)

[1] Born in Olinda, Calif., Dec. 24, 1906. Graduated in engineering from Stanford University in June 1928. Married Beatrice Brailsford in May 1929, a graduate of Stanford University.

Employed by Richfield Oil Corp. in December 1928 and worked as a development engineer until August 1939. Field superintendent until April 1945, then as assistant manager of production until January 1953 when elected vice president and manager of production. Mr. Travers was appointed a director in April 1957.



MAP OF ATHABASKA TAR-SAND AREA

SHOWING APPROXIMATE DISTANCES FROM CITIES AND VILLAGES OF PROPOSED SITE.

NOTE: ALL DISTANCES IN MILES.

Mr. TRAVERS. Do you mean the location of the sands on the map?

Representative PRICE. Yes.

Mr. TRAVERS. They are located in the northeasterly portion of the Province of Alberta, about 200 miles north of Edmonton, Alberta.

Representative PRICE. Thank you.

Mr. TRAVERS. While it has long been known that there are large quantities of oil sands beneath the Athabaska area, the full magnitude of these deposits has been determined only in recent years through the drilling of over 1,000 shallow test wells to determine the thickness and saturation of the sand by Richfield and its associates, and by numerous other oil companies.

Geologic and stratigraphic data from these wells indicate that the oil sands underlie at least 17,000 square miles, and we think may contain as much as 600 billion barrels of oil in place. If as much as one-half of the Athabaska oil can be recovered, the quantity will be equal to the present known recoverable reserves of the rest of the world.

These oil sands crop out along the banks of the Athabaska River. From here they dip westerly and become covered with up to 2,000 feet of overburden. In fact, only 2 to 10 percent—depending upon the cost of overburden removal—of this deposit has a sufficiently thin overburden to permit oil recovery by the employment of open-pit mining methods.

Several experimental methods have been tried to free the oil from these deeply buried deposits where the reservoir temperature ranges from 40° to 75° F. in deeper portions. In this temperature range, the oil is too viscous to flow freely and, therefore, plugs the porosity of the sand. This immobility of the viscous oil has impeded all attempts by conventional methods such as circulating hot gases and fluids through the sand.

With the advent of nuclear explosives, man now has available a very large source of energy in a package small enough to be placed deep underground through a drill hole. This energy, when released in or adjacent to a deeply buried viscous oil deposit, will lower its viscosity, thereby increasing the effective permeability of the sand through which the oil must flow to recovery wells.

At a test site near Mercury, Nev., the Atomic Energy Commission has exploded several nuclear devices at underground depths sufficient to contain all the effects of the blast. Based on data derived from these contained explosions, it is reasonable to expect that the following results would be obtained if a 9-kiloton device can be exploded below the oil sand bed at a depth of about 1,300 feet.

The explosion would suddenly liberate 9 trillion calories of heat energy which would force the oil sand upward and create a slag-lined sphere about 230 feet in diameter. After cooling a few minutes, this sphere would collapse and a very large amount of oil sand would fall into the cavity. Here it would be heated to temperatures which would lower the viscosity of the oil so that it would be produced by conventional pumping or flowing methods.

The proposed detonation would be completely contained so that no radioactive materials would vent into the atmosphere.

Data from the aforementioned underground explosions at the Nevada test site have confirmed the validity of the formula for com-

puting the depth at which an underground explosion will not vent to the surface. For the material at the Nevada test site, the formula developed was depth equals 450 times the cube root of W where W is the force of the explosion expressed in equivalent kilotons of TNT.

$$(D=450\sqrt[3]{W})$$

This formula is now believed to be somewhat on the conservative side.

According to this formula, no venting to the surface would occur if a 9-kiloton device were detonated at a depth of 950 feet. In the proposed experiment, a safety factor of more than two is applied by placing the device below the oil sand at a depth of 1,300 feet.

The Nevada data indicate that there will be no venting to the atmosphere of radioactive debris and, therefore, it is believed that no question of fallout is involved in this experiment. It is also believed that the oil and ground water will not be contaminated by radioactive material.

As a result of extensive research, a good deal is known about the disposition of radioactive fission products produced by a fully contained underground nuclear explosion. Between 50 and 90 percent of these fission products are dissolved in the cavity lining formed by the cooling of the melted rock. Their retention in the slag reduces their availability to contaminate oil or water.

The fission products not dissolved in the slag lining at the time of cavity collapse take the form of gases, chiefly radioactive isotopes of krypton and xenon. These isotopes have half lives of less than a few minutes, but this is long enough to prevent a portion of them from being dissolved in the slag and to allow some of the radioactivity to permeate the broken rock of the cavity.

The radioactive krypton and xenon decay rapidly to the longer lived radioactive elements strontium 90 and cesium 137. By a process of ion exchange, which was described very well, I thought, by Dr. Brown, these two elements take the place of other elements in the medium and are retained by rock minerals in the vicinity of the explosion.

The net effect of all these factors is that there would be no contamination of oil or ground water beyond acceptable limits.

Furthermore, the results of a hydrodynamic study in the proposed test site area indicates that there are no pressure gradients in the aquifers which might be involved, which would cause the water to move rapidly through them.

Calculations indicate that the rate of water movement would be on the order of a few feet per year. Consequently, any radioactive materials deposited in an aquifer near the detonation would decay to impotency before they could move more than a short distance from their point of origin.

Athabaska oil irradiated in the Oak Ridge reactor for longer periods of time than would result from an underground explosion did not retain objectionable levels of radioactivity.

As a safety precaution, the area surrounding the test site would be monitored for radiation before and after the detonation. All underground aquifers will be tested for radioactivity before and after the detonation.

The seismic effect is not a hazard in this area. The seismic shock resulting from an underground detonation of the size proposed for this experiment is quite strong on the surface immediately above the detonation point, but rapidly decreases with radial distance from that point.

The nearest installation to the proposed test site is a railroad siding about 6 miles away, and at this distance the seismic effect is expected to cause no damage. The nearest town, Fort McMurray, which is 64 miles away, should feel no shock whatsoever.

The exact time of detonation would be communicated to monitoring stations within possible range so that sensitive seismic equipment would be able to gain valuable scientific data from the shock.

In summary, based on available information, Canadian and United States scientists who have carefully studied the safety problem are convinced that the proposed 9-kiloton test would not result in harmful effects.

After considerable investigation, the Athabaska area of Alberta was selected as the most desirable site for the proposed experiment for the following reasons:

1. The area is remote and uninhabited, with no surface culture such as farms, dwellings, or towns.

2. There are no developed oilfields which could be disturbed by the seismic effects produced by an underground explosion.

3. The lands involved are all crown lands under the administration of the Provincial government of Alberta, thus simplifying operational and legal matters.

4. The estimated amount of oil is sufficiently large to warrant the test.

5. The oil sands are buried deeply enough to permit containment of a nuclear explosion of sufficient energy yield to make the experiment economically attractive.

6. There is no known deposit in the United States which satisfactorily meets these conditions.

Richfield has been actively interested in this project for over 4 years. We have recently entered into an agreement with Cities Service Co. and Imperial Oil, Ltd., to carry on the experimental work.

Our group has worked not only with the scientists of the Lawrence Radiation Laboratory, but also with scientific committees in Canada. Discussions so far have dealt only with scientific and technical phases of the proposed experiment.

The technical committee appointed by the Department of Mines and Technical Surveys of the Canadian Government has not yet filed its report. Therefore, our group has not yet made any application to that Government for the conduct of the experiment.

The experiment here proposed would be performed by the Atomic Energy Commission, but the private companies involved propose to reimburse the Atomic Energy Commission for the cost of the material used and the time and expenses of the personnel necessary to the experiment.

Petroleum is in temporary oversupply, but demand is rapidly growing and may change this situation sooner than we can now foresee. From that point of view it would be desirable that work on the proposal start as soon as possible, for this is a long-range project in a new

field of research. Many years of testing and construction will be necessary before commercial production could be achieved.

In conclusion, here in the Western Hemisphere controlled by a friendly neighbor is a very large deposit of petroleum. If and when the Canadian authorities request the U.S. Government and Richfield Oil Corp. to undertake the experiment we have discussed, and if the experiment indicates that it is possible to produce oil from the Athabaska sands with the aid of nuclear heat, it will make a major contribution toward supplying our ever-increasing energy needs.

Thank you very much for this opportunity to present our views.

Representative PRICE. Thank you.

In this map, when you showed the area of the proposed experiment, how large an area is that?

Mr. TRAVERS. The area of the deposit?

Representative PRICE. Yes.

Mr. TRAVERS. It is 17,000 square miles, about 10 million acres.

Representative PRICE. What effect would the planned operation have on the future use of the area, say, for farm purposes, or perhaps to the future establishment of a town in the area?

Mr. TRAVERS. It is not expected that any of these explosions will vent at the surface or produce any marked disturbance or subsidence at the surface. At present the areas are a frozen wilderness in the wintertime, and muskeg, or practically bottomless mud pits, in the summer, and it is hardly likely that the area will ever be drained to the point where it could be farmed or used for any other of the normal purposes for which the surface might be used.

Representative PRICE. Could chemical explosion experiments be used to obtain the data that you are seeking to prove out this technique?

Mr. TRAVERS. We have considered attempting to experiment with high explosives, but within the limitations with which you have to work—in other words, the bore hole size of a drilled well—it is not conceivably possible to place enough high explosives to produce a result which would have attractive economic possibilities.

Representative PRICE. I know that you are just getting into research now, and what will come after the explosion and the experiment in the field, but do you think the motor fuel refined from the oil obtained from the experiment will have any atmospheric contamination?

Mr. TRAVERS. We are certain that the oil would not retain any of the radioactive materials. We took a sample of oil that we recovered from the Athabaska sands and irradiated it at Oak Ridge, and by the time we got the sample back to Los Angeles it was back to background level and had no detectable retention of radiation effect in the oil itself.

Representative PRICE. That prompts me to ask a question about the test you made at Oak Ridge. That was in the crude-oil stage and not the refined stage; wasn't it?

Mr. TRAVERS. Yes.

Senator PASTORE. How do you explain that? You say there would be no contamination in the oil itself, caused by these bomb explosions. Would you explain that?

Mr. TRAVERS. I would rather defer that to someone like Dr. Teller, who understands why this is much better than I do. But there are

no minerals or other materials in the oil which have the property of retaining the effect of radiation.

Senator PASTORE. That will be explained this afternoon, will it?

Representative PRICE. Dr. Teller is here and we can ask him.

Chairman ANDERSON. You say it is also believed that oil and ground water will not be contaminated by radioactive material.

I am happy to go along with it as "also believed," but would it not be wise sometime to be able to say that "It has been proven"? A good many people have been writing in saying, "They are proposing to contaminate all the water supply in the country."

I have had a recent letter from New Mexico suggesting to me that the Project Gnome, which is in a way similar to yours, is going to contaminate all the ground water in our part of the country. I firmly believe that it will not.

Is there some way that we are going to be able to test this beforehand? If not, then is there not great significance in the Athabaska test because it gives us a chance to prove or not to prove whether this is true?

Mr. TRAVERS. It would give us that opportunity in an area that is uninhabited. We are now, during this present winter, working on a program with the scientists in the Provincial Government of Alberta to demonstrate that this contamination cannot be transmitted from the site of the explosion to any spring or outcrop where the water might be used and result in a harmful effect.

I think by the time this winter's work has been completed, the Alberta people will be completely satisfied on this point. All the data, incidentally, that have been gathered so far are in the large reports which have been submitted to the members of the committee.

Chairman ANDERSON. You do see a good byproduct from the first shot that would be made, that there will be some real value in showing that the oil and the ground water will not be contaminated?

Mr. TRAVERS. I surely believe so, and I surely think that this is one of the things we would hope to demonstrate.

Representative PRICE. Are there any further questions?

Mr. Westland.

Representative WESTLAND. You said you would produce a cavity of some 250 feet, or so. How much oil would you expect to get out of that from a 9-kiloton blast?

Mr. TRAVERS. This is not known, of course, with any definiteness. If we knew, we would not have to do it. But from the effect of nuclear heat, alone, we expect to heat a rather large amount of oil. It is indefinite. We just do not know exactly by what mechanism the heat will be distributed.

But we think it will be where a 9-kiloton blast would yield the order of several hundred thousand barrels from nuclear heat alone.

Representative HOSMER. Several hundred thousand barrels?

Mr. TRAVERS. Yes. Of course, there are other mechanisms by which the energy available to us in the form of a blast can be distributed and used to crack and lower the viscosity of the oil. We simply do not know the answer as to how widespread the effect will be and what quantity of oil will be liberated by any specific size detonation.

Representative WESTLAND. A couple hundred thousand barrel well is not a very productive well, is it?

Mr. TRAVERS. Well, it would be an average well, I would say, a total recovery of that amount.

Representative WESTLAND. In other words, if you did produce a couple hundred thousand barrels of oil from the first one, you would consider that, then, to be reasonably satisfactory?

Mr. TRAVERS. We would consider it to be encouraging, and probably not economic because of the unusual costs attendant on the first experiment. But I think we would gain information that would lead us to draw a conclusion as to whether or not the process could finally be made economic.

Representative WESTLAND. In other words, then, in order for this to really work, you would have to do a great many of these blasts?

Mr. TRAVERS. Our agreement and our association with the other two companies contemplates a series of probably at least three in varying sizes and placed in varying positions. And, of course, more than this may be required.

Representative WESTLAND. And you get what—a couple dollars a barrel for that oil, or it is worth that?

Mr. TRAVERS. Yes, sir; approximately $2.

Representative WESTLAND. There has been a lot of oil exploration up in that area. I noticed in your statement you do not believe any of the results of this explosion would be detrimental to other presently operating fields.

Would you have to contain this area, then, to a rather limited extent, Mr. Travers?

Mr. TRAVERS. We have explored this area with a great number of wells, as I mentioned. There is no oilfield close to the proposed test site.

Representative WESTLAND. There may not be oilfields, but many, many companies have been prospecting, let us say, in through that area, have they not?

Mr. TRAVERS. Yes, quite a large number, but there is no liquid oil that has ever been discovered in the area. There are no facilities in place for the recovery of oil by normal methods.

Representative WESTLAND. But you feel within the areas that you now have, Richfield, Imperial, that you could conduct these experiments and, let us say, retain the seismic waves within your own area; is that correct?

Mr. TRAVERS. Within the permits which we hold, yes, very definitely we feel that.

Representative HOSMER. Mr. Chairman——

Representative PRICE. Mr. Hosmer.

Representative HOSMER. I am rather shocked at the small quantity of oil that you anticipate liberating through one of these explosions. I had the staff calculate the capacity of that 230-foot-diameter sphere. We may have calculated wrong, but it would amount to about 6 million barrels of capacity, from our calculations, into which you described the oil sands as tumbling and becoming viscous and subject to extraction.

Would you like to clear this up a little bit?

Mr. TRAVERS. In the first place, of course, you must remember that the hydrocarbon content of the sand is of the order of 10 percent of the total volume.

Representative HOSMER. That brings us down, then, to 600,000 barrels.

Mr. TRAVERS. And we think perhaps we would recover half of it.

Representative HOSMER. That would bring you down to around 300,000, at $2 a barrel, to $600,000 recovered.

You have your original hole you would have to drill to put your device in. After you fired the device, you have to drill the second hole to extract the oil; is that correct?

Mr. TRAVERS. Yes; at least one.

Representative HOSMER. Were you present when Dr. Abelson gave his testimony?

Mr. TRAVERS. Yes.

Representative HOSMER. He described just by way of example some chemical activities which could occur from the heat created. You have described the release of 9 trillion calories in this 230-foot sphere, which is 10 percent hydrocarbon and 90 percent sand silicate. I think Dr. Abelson in some of his chemistry had some silica.

What is going to prevent us from ending up with the chemicals that he mentioned as a result from one of these explosions, rather than the result of merely a viscous fluid to reach the surface?

Mr. TRAVERS. It is quite possible that we will vaporize some of the rock minerals, but that, of course, would be one of the objects of the experiment, to determine where to place the device to liberate as little as possible.

Representative HOSMER. I do not think there is any doubt that you are going to vaporize some of these things. My question is directed to the chemical reactions which will occur as this mass cools, whether the results will be fluids or some other combination of substances.

Mr. TRAVERS. We frankly do not know. We may permanently gasify a large portion of the oil. It is unclear at this point by what mechanism the heat will be distributed and what will happen as it cools. There is no way of predicting that.

Representative HOSMER. Mr. Travers, you have done a pretty good job of estimating the occurrences up to the moment of explosion, including the moment of explosion, and thereafter with respect to the factor of noncontamination of the general environment.

You must have some calculation with respect to these other factors about which I have questioned you.

Mr. TRAVERS. We have done a lot of thinking about it, and we have done all the calculating it is possible to do, I think. Of course, from one point of view it would be desirable to use a much larger device and bury it much deeper. But this would also have some objections, I am sure.

Our present thinking is that if we are limited, for example, to about 20 kilotons, which has been discussed as a possible limit for explosions of this kind, it may be necessary to expand the area, but by using two or more devices placed some distances apart, but detonated simultaneously.

In fact, our present thinking is that several small devices may be preferable to one large device.

Representative HOSMER. There are considerable economic variations with the use of several smaller in relation to one large device.

Mr. TRAVERS. There may be. We have as yet received no definite

information as to the cost of the devices. The cost of placement would not be formidable, but the cost of the devices we have no real information on.

Representative HOSMER. You have described the limitations on surface mining, because of the fact that the sands are only at the surface in a fraction of the field. There must be some similar limitation, then, upon the nuclear extraction procedure if that must be done at certain and particular depths.

Do you have any calculations for that?

Mr. TRAVERS. From our present thinking, there would be a band, the volume of which I have not calculated, which would be below mining depths but still too shallow to contain a sizable nuclear blast. The larger portion, however, of the sands are buried 1,000 feet or more, so the containment would be possible.

We also have in mind, of course, the possibility of expanding the effect of the nuclear detonation by conventional means. For example, in a small borehole it is extremely slow to try to circulate enough hot fluids in to melt the surrounding walls with any satisfactory rate of progress. But if you could start in a cavity 200 feet across, perhaps the conventional means of expanding the effect would become much more attractive.

Representative HOSMER. Mr. Travers, I think in light of all these difficulties and unknowns with which you are dealing here, the men in your company and your company are to be congratulated on taking on the difficult, the expensive, and possibly unremunerative experiments that you have.

Mr. TRAVERS. Thank you. We feel that it justifies a major expenditure on our part.

Representative PRICE. Thank you very much, Mr. Travers. The committee appreciates your cooperation and your fine statement.

Mr. TRAVERS. Thank you.

Representative PRICE. The next witness will be Dr. James Boyd.

Dr. Boyd, you may proceed with your statement.

STATEMENT OF JAMES BOYD,[1] KENNECOTT COPPER CORP.

Dr. BOYD. Mr. Chairman and members of the Joint Committee on Atomic Energy, my name is James Boyd. I am the vice president in charge of exploration for Kennecott Copper Corp., a former Director of the U.S. Bureau of Mines, and currently the president of the Mining and Metallurgical Society of America. I am a graduate engi-

[1] Born: Kanowna, western Australia.
Education: Preparatory schools in Australia and England; Hollywood High School, California; B. Sc., California Institute of Technology (1927); M. Sc., Colorado School of Mines (1932); D. Sc., Colorado School of Mines (1934).
Experience: From 1927 to 1929, exploration geophysics with Radiore Co., a consulting firm; from 1929 to 1941, carried on consulting practice in mining, oil, and geophysics, concurrently with teaching and graduate study. In 1941 was inducted into the Army with rank of captain, working in the Office of Under Secretary of War on strategic minerals activities of the Munitions Board. Army representative on War Production Board's Program Adjustment Committee. Later executive officer to Director of Materials ASF. Served on the Combined Raw Materials Board Advisory Committee and in 1945 reported to Gen. Lucius D. Clay in Europe as Director of Industry. Discharged with rank of colonel in 1946.
After serving as dean of the faculty at Colorado School of Mines, became assistant to the Secretary of the Interior and in August 1947 was appointed Director of the U.S. Bureau of Mines. From 1949 to 1951 was concurrently Defense Minerals Administrator.
Since October 1951 has been associated with Kennecott Copper Corp., first in the capacity of exploration manager and since January 1955 as vice president, exploration; 1954 to 1956, Chairman of the National Science Foundation's Committee on Minerals Research; 1960, president of the Mining and Metallurgical Society of America.

neer-economist from the California Institute of Technology, and hold advanced degrees in geology and geophysics from the Colorado School of Mines.

As a nation we have only just begun to experiment with the knowledge gained from the successful control of nuclear energy.

Very naturally all thinking men, when confronted with this information, immediately begin to meditate on how it might be applied in their own fields. We all can have ideas; the mass application of human minds inevitably produces a proliferation of theories. Many, on how nuclear energy may be used in mining, have been pronounced—some fanciful, some down to earth, but all controversial. Theories inevitably remain doubtful until they can be backed up by demonstrable facts. It is through the pronouncement of theories that thoughts are stimulated to practical reality.

Controversy is essential to make men think clearly and practically and apply their knowledge to useful things.

The scientists charged with responsibility for Project Plowshare and others have shown considerable vision in recognizing the fields in which nuclear and thermonuclear devices may be useful. The tremendous energy instantaneously released by such devices is almost beyond imagination even to miners who use the conventional type of high explosives every day. By the same token, those who are more familiar with the devices do not have a background of experience in mining.

I do not appear before you, therefore, as an expert on the peaceful application of atomic energy in mining. I am not sure that at this stage of our knowledge any man can claim to be an expert. Perhaps some, through study, can claim to have a slightly lesser degree of ignorance than others. My testimony here today can add very little which is new, for many minds have been applied to the problem and my knowledge is mainly a reflection of the thought and contemplation of others. I would venture to say that despite the number of ideas thought of to date, many that eventually will be applied probably have not yet been conceived.

Before discussing any of these ideas, I would like to say a few words concerning the present status of mineral supply, for this has a bearing on the subject under discussion.

Through the efforts of the Congress, the executive branch, and the various industries involved over the past few years, there is virtually no mineral raw material or fuel that is not in ample supply to meet the voracious needs of the entire free world's industrial economy as it stands today. The problems that are brought to Congress in these areas are the result of overcapacity to produce—not shortages.

In other words, for a short time at least, we did too good a job. Through stimulation to meet the shortages resulting from the past two wars, we overshot the mark and created new problems for ourselves. For a hundred years this country's growing economy was materially aided by skimming off the cream of natural resources to supply not only ourselves, but much of the rest of the world. Now we find that some of these countries are increasing natural resource production and export to support their own growth; hence, some of our extractive industries are competing with skimmed milk against cream. This by no means infers that we are running out of re-

sources; it emphasizes only that the problems are economic and social, not material.

Let me illustrate with my own industry. At the turn of the century the average grade of copper ores mined in this country was about 2½ percent or 50 pounds of copper per ton of ore. From this we supplied ourselves and a large part of the industrialized world. Today the average grade of our domestic mines is less than 1 percent or about 19 pounds per ton. The growth of the economy has been such that, although we are producing more total copper than in 1900, we are importing as well. Our mines compete with 30- to 40-pound ores being mined in Chile and 60- to 80-pound ores in the African copper belt, the other two big producing areas.

I have dwelt on this matter because it provides the basis for the subject under discussion. The conservation of our resources is intimately related to economical extraction. The costs of recovery lie in mining, concentrating, and refining. The latter two are being constantly advanced through research in improved technology. They do, however, require large amounts of energy and, when deposits lie at some distance from sources of fossil fuels such as coal, oil and gas, may eventually depend upon atomic power.

The immediate consideration is that of improving the costs of mining. Mining companies are constantly striving to do this. Most of these improvements are small; a saving of a half cent per ton is important when applied to a production of tens of millions of tons per year. One of the outstanding economies adopted in recent years has been the use of ammonium nitrate as the explosive in most open pit mines. It is only occasionally that a radical departure from former practice occurs. Probably the two outstanding examples in this century are the emergence of large open pit mines and the underground block-caving mines.

As the future reserves will be found to lie deeper beneath the surface, and the metal content of the ores probably will be lower, the cost of mining must inevitably increase unless we find less expensive ways of doing it. The theories as to how we can do this by the use of atomic energy revolve mainly around the use of explosive devices, because the breaking of rock is the important step in mining—both in removing the overburden and in mining the ore itself. It is not just a matter of breaking the rock, but breaking it so that the fragments can be readily handled and hauled to the mill. Briefly, the problem is whether or not nuclear blasts can do this more economically than conventional methods, yet safely and without contaminating or diluting the ore bodies.

The theories involved in breaking large masses of rock by means of nuclear blasts have been the subject of several papers by contractors for the Atomic Energy Commission. These are largely based upon experiments with large blasts of conventional explosives, and the results of the first underground atomic detonation in Nevada. I am sure these first approaches have raised more questions than they have answered, but this is the way of research. In total, they point out the feasibility of breaking large masses of rock fairly cheaply without serious danger from radiation at the surface.

The conclusions are that this sort of blasting might lead to several things, some of which are:

(1) Breaking and removal of overburden before the mining of the ore body is undertaken.

(2) Breaking of the ore in preparation for open-pit mining.

(3) Breaking of ore and cap rock to facilitate block caving.

(4) Breaking of ore to permit the circulation of solutions through the broken ore in order to extract the valuable minerals chemically.

(5) Breaking of oil shale to permit its being retorted underground without the necessity of mining.

Some of these ideas, such as breaking ore to be handled subsequently by conventional methods, might result in a minor but important economy. Others, such as instantaneously blowing all of the overburden off a buried deposit, would be revolutionary changes.

Although each suggestion is backed by theoretical calculations, the authors point out many unknown factors which must be determined by future experimentation. When discussed with experienced miners, many additional considerations are raised.

The breaking of oil shale to permit its being retorted underground has probably had as much serious, thorough consideration by knowledgeable people as any item in the Plowshare program, yet there are many uncertainties. As expressed by Walter I. R. Murphy of the Bureau of Mines:

> * * * many people are still unaware that there are two parts to this project, the second being a study of methods for producing oil by in situ combustion of the broken shale after the explosion. * * * It is more difficult to discuss than the nuclear phase, because plans must remain nebulous until results of the explosion are known.

Mr. Murphy's expression of the problem is generally applicable to the various proposals. There is always the "second part" which cannot be planned until we know how the first part is going to turn out. The force of a nuclear explosion is such that we can be sure great physical changes and some chemical ones will take place in the surrounding rock. But what will the changes be? Will they be advantageous to our purpose, or will they create new difficulties and hazards?

The conclusion from all the discussions seems to be that there is sufficient evidence that nuclear devices will eventually be used to break rock to warrant extensive research. It is realized, of course, that because of international agreements, Plowshare has been unable to detonate a single nuclear device for its own purposes.

Even when nuclear blasts can be detonated, we cannot experiment with actual ore bodies until we know much more about what we will be doing. These ore bodies are costly to find and constitute the basic wealth of the Nation. To experiment on these bodies of ore with the present state of our knowledge would be to gamble unnecessarily with our birthright. We therefore will have to do this research in barren rocks which have the same physical characteristics as the ore bodies. The very valuable "Rainier" shot was deliberately set off in a spongy volcanic rock for safety's sake. Actually, it was primarily designed to demonstrate the feasibility of containing a nuclear explosion. The benefits to mining were incidental.

Rock, from the miners' viewpoint, is not just rock; it is a whole host of different types of material with vastly different physical characteristics. Before we jeopardize the valuable deposits, we must know

more accurately what will happen under a variety of different geological conditions. Thus the planning for the costly experiments that are entailed must be done by teams of experts in a great number of fields of knowledge, as well as by the atomic scientists themselves. These are very practical matters and more than theory is needed in planning experiments.

We should learn from the mining engineers and the geologists where to look for experimental sites with conditions similar to those that are to be found in various types of ore bodies which could possibly benefit from the use of these methods. The site itself must be as thoroughly studied to determine the exact conditions to be encountered as would be done by a mining company preparing a deposit for operation.

Because it is as much an economic problem as a scientific one, the costs of site preparation must be carefully determined and the experiments carried out as economically as possible. The results must be carefully studied to determine the amount of rock broken, the size of the fragments, and the distribution of the radiation products. Tests will need to be made to determine the feasibility of using methods of directing the forces of the explosion to accomplish the desired results.

It is vitally important in the national interest that research money not be wasted. Hurried, unplanned decisions to set off one of these devices will not obtain for us the full benefit of the money spent, and all too frequently disappointing results can put off the day when we will be able to harness this powerful tool in the interests of the Nation. Research rarely brings quick and spectacular results; it involves endless painstaking work. An understanding Congress is needed to provide the funds for the kind of basic research required to solve these problems, in full knowledge that it may take several years to obtain the essential data. It has taken a hundred years of research applied to actual practice to bring the use of conventional explosives to the present state of perfection. In this case, much of the experience gained with so-called high explosives can be utilized in controlling the greater forces of the atom, and it certainly should not take a hundred years to find important uses.

This is not to say that careful planning has not been put into the few experiments proposed to date, but we have in some cases detected an impatience to get spectacular results that has prevented using the knowledge of all the people in Government and industry competent to contribute to the full utilization of these experiments.

Extensive panel discussions were undertaken in planning for the experiments on the extraction of shale oil, and the preparation for these experiments is much further along than the others. This is primarily because the U.S. Bureau of Mines, the Geological Survey, and industry have been working together for some 15 years on these and related problems. From the published papers and our knowledge of the present state of joint efforts, the same is not true in the field of mining metallic ores; this is partly because the problems involved are more complex and there has not been as much effort spent in experimentation.

I am confident, however, that with patience and painstaking efforts, much can be done to improve the economics of mining. Actually there is time to take this kind of care as long as the work is projected vigor-

ously, for, as I said earlier, our economic problems are currently those generated by surpluses and not shortages of raw materials. We should make a start, however, because time has a habit of passing quickly, and 10 years are not long in the history of the Nation.

In summary, I should like to say that considerable ingenuity has been shown in visualizing possible uses of nuclear and thermonuclear devices in mining applications; many other undoubtedly will be conceived. Our mineral resources are wasting assets which cannot be replaced once they are mined, and any effort to conserve them is vital to the long-range health of the economy. Before such applications can become realities, however, much experimental work remains to be done. Although there is no immediate, urgent economic need for completing the work, the experiments should be proceeding so that the basic data will be available when required. In conducting these experiments, the long experience of the mining industry should be utilized to the fullest possible extent.

Thank you.

Representative PRICE. Thank you, sir. That was certainly a fine statement.

Are there any questions?

Chairman ANDERSON. Dr. Boyd, on page 8 I appreciate particularly the care with which that has been stated to us, that before we jeopardize the valuable deposits, we must know more accurately what will happen under a variety of different geological conditions.

That is one reason why we have to start sometime with these tests. But we must do it with extreme care. That is why I have been very much interested in the work the Atomic Energy Commission has planned in my part of the country, and I guess in other parts of the world, because they are planning them with extreme care, to know what they have found out when they have found it out, and they will not disturb the deposits in the other parts of the country.

I am very glad you issued that extra note of warning.

Dr. BOYD. Thank you.

Representative PRICE. Are there any further questions of Dr. Boyd?

If not, thank you very much, Dr. Boyd. It is certainly a valuable contribution to the hearing.

(Several questions were submitted to Dr. Boyd after completing his testimony. The questions and Dr. Boyd's answers follow:)

KENNECOTT COPPER CORP.
New York, N.Y., March 28, 1960.

Mr. J. T. RAMEY,
Joint Committee on Atomic Energy,
Capitol Building, Washington, D.C.

DEAR MR. RAMEY: Attached are my answers to the questions you posed, but which the committee did not have time to ask. I trust they are sufficiently responsive.

Sincerely,

JAMES BOYD.

QUESTIONS AND ANSWERS TO ACCOMPANY TESTIMONY PRESENTED BY JAMES BOYD TO THE JOINT CONGRESSIONAL COMMITTEE ON ATOMIC ENERGY, MARCH 22, 1960

Question. Are there specific minerals, etc. which should be exploited first by this proposed method? Would not this method be limited to use in regions which are not populated and not near operating mines, etc?

Answer. In general the use of nuclear explosives in mining probably will be applicable only to minerals which occur in huge tonnages such as iron, copper,

molybdenum, and oil shale. Whether or not it will ever be possible to use nuclear explosives in a mine near a population center will depend on our ability to learn how to eliminate radiation. The inherent dangers are the seismic effects and radiation. The answer can only be determined experimentally. Certainly the experiments should be conducted in remote, unpopulated areas.

Question. You infer that much work has to be done in locating sites for tests which will give significant results. Why hasn't this work been done already?

Answer. As a member of the mining industry, I have tried to keep informed of the progress made in the Plowshare program. Just why extensive testing has not been done of sites in barren rock with physical characteristics similar to those of the rocks containing ore deposits I do not know.

Question. What specific experiments should be proceeding to obtain the basic data you refer to?

Answer. It is not possible in a brief presentation to go into much detail. Therefore I should like to outline for you some of the problems that need to be solved before nuclear devices can be used in mining.

I. FACTORS WHICH WILL AFFECT THE ADAPTABILITY OF NUCLEAR DEVICES TO MINING

A. Nuclear explosives characteristics

1. *Distribution of energy.*—The energy yield and its partition in a nuclear detonation and the relation of this yield to the energy produced in the detonation of conventional explosives requires further clarification. It has not been firmly established that a realistic correlation of the two forces exists. This is a subject of some confusion to the industrial engineer who has worked only with high explosives.

2. *Specifications.*—Device dimensions must be defined to determine what the technical difficulties and costs will be in placing the explosive in firing position. It is evident that sinking a small-diameter shaft or drilling a well to shallow depths in relatively soft rock for device emplacement may not pose any engineering problems or be exceptionally costly. But an intricate emplacement tunnel, shaft, or well arrangement of large size at several thousand feet in hard rock poses an entirely different problem of abilities and expenses. It thus becomes obvious that, in terms of emplacement, the smaller the device per unit yield, the more practical its use. Therefore, it is impossible to deal in nebulous terms about the dimensions of a potential industrial explosive.

3. *Multiple and directional charges.*—Simultaneous detonation of several devices in a given type of rock, distribution of the explosively linearly along a drill hole or mine working, and multiple delayed-timing detonations are possibilities to be investigated. The shaped charge—that is, directing the force of the explosive along predetermined planes—could play an important part in the adaptability of the nuclear device to industrial applications.

4. *Liability and personnel.*—Safety and simplicity in handling and charging the nuclear explosive will be a major issue of potential users. Can mine labor under conventional engineering and managerial supervision handle the devices? Will nuclear engineers, health physicists, and the like have to be employed by companies using nuclear explosives? Insurance liability protection, the union's interests in employee welfare, and many similar sociological factors are extremely important. A detailed and accurate picture of the safety precautions and specialization should be developed.

5. *Costs.*—The cost of the nuclear device, its value per increment of yield, charges for emplacement under varying conditions, and the expenses of associated safety investigations must be known in more detail.

B. Rock characteristics

1. *Properties.*—The physical and chemical characteristics of rock formations and minerals will influence and be influenced by the detonation of nuclear explosives therein.

Mechanical properties of rock that will influence the reaction of nuclear explosives are specific gravity, density, porosity, strength, hardness, and elastic and plastic qualities.

The more common chemical compounds that will be encountered in ingeneous formations are silica and alumina, accompanied by iron, calcium, magnesium, sodium, and potassium, with trace amounts of less common elements. Calcium and magnesium carbonates predominate in sedimentary rocks like limestone and dolomite; and residual rocks, like sandstones, have compositions similar to igneous rocks. Special mineral deposits may contain most any of the metallic

56108—60——4

elements, but, of those which may occur in deposits large enough for mining by nuclear explosives, iron and copper predominate.

The associated constituents of formations that could be mined with nuclear explosives include water, which will occur virtually universally, and carbonaceous material—particularly coal, petroleum, and natural gas.

Predetermination of these characteristics, accompanied by a knowledge of the nuclear reaction, will enable industry to determine some of the advantages and disadvantages of using the device.

2. *Structures.*—Structural features such as joints, fractures, bedding planes, faults, and voids in rock formations will influence the results of underground nuclear explosions. One or more of these structural features are present in all formations and they constitute planes and areas of weakness that cannot be overlooked in mining operations.

Weathering and chemical alteration of rock should be mentioned. Weathering extends to great depth in some areas and may create a preamble blanket of unconsolidated rubble and broken rock. Chemical alterations affect the mechanical properties of the rock, usually softening and weakening the formation, although the reverse may also occur in some instances. The extent and degree of weathering and alteration, if not recognized and explored, may result in unexpected consequences in the use of nuclear explosives.

To the extent possible, complete knowledge should be acquired on the character of any specific formation or deposit considered as a site for nuclear detonation, for the introduction of such explosives will present problems not heretofore encountered in mining.

II. INFLUENCE OF FACTORS UPON APPLICATION OF THE NUCLEAR DEVICE

A. Containment

1. *Burial.*—The depth of burial of an underground explosion determines the amount of rock breakage at the surface and the degree of confinement for the explosive gases and radioactive debris. Results from both nuclear and HE explosions have shown that the depth of burial is a scaled quantity. The larger the scaled depth of burial the less breakage of rock at the surface. Results of HE testing in various rock types have shown that surface rock breakage can be classified into three main categories: (1) Crater formation back to charge point; (2) crater formation to a depth less than charge depth; and (3) surface cracking but no crater. Furthermore, the value of the scaled depth of burial to achieve these various results has been shown to be a function of rock type.

2. *Prediction.*—It has not been demonstrated satisfactorily to date that these scaled depths may be predicted accurately from physical property measurements of the rock or from HE testing in the formation. This disagreement between HE tests and nuclear tests probably results from the fact that there is surface weathering of the rock which affects the shallow HE explosion but does not affect the more deeply buried nuclear explosions.

B. Shock effects

1. *Seismic disturbances.*—In all directions from the detonation of any explosive underground, seismic disturbances will travel radially outward. The magnitude, rise time, basic frequency, and duration of these seismic disturbances at various distances from the source are important for predicting vibration effects upon underground opening and structures, water, oil and gas wells, and surface installations. Considerable effort is being expended to measure the vibrational effects of underground nuclear and HE detonations. However, at this time, there does not appear to be available a method of correlating satisfactory vibrational data from HE and nuclear tests, the reason being that equivalent data in like rock types are not available.

2. *Crushing and fragmentation.*—The pressure in the shock front of the detonation falls rapidly as it progresses from ground zero. As the shock front moves outward, the cavity must expand radially, forming a crushed zone. Beyond the crushed zone the shock wave probably decays rapidly to an elastic compressive pulse which is transmitted outward through the rock without causing damage unless it encounters free faces or boundaries where reflection can occur, such as faults, fractures, tunnels and shafts. The distances at which these effects will take place will depend upon rock conditions. The size of the cavity, extent of crushing, and the degree of fragmentation probably will be influenced by such structural features as porosity and jointing and fracturing in the subject formations. How accurately the extent and condition of these

zones can be predicted from theoretical considerations and from small-scale detonations has not been established.

It is necessary to emphasize that the degree of fragmentation is extremely important, for it is always desirable to break the rock into small sizes. In extractive processes it is also necessary to leave the broken material in a permeable condition. Boulders 5 or more feet in diameter will not prove very helpful, but fragments measuring 5 inches or less would be attractive for recovery and processing.

3. *Caving.*—Collapse about ground zero will be determined by the size of the blast cavity and the strength of formations forming the arch over the cavity. The natural expansion of broken rock will limit the quantity of rock in the collapse to something less than the tonnage of solid rock which occupied this space prior to the blast. The broken, fragmented rock in the column above the cavity is not produced by the explosion, but by the inability of the overlying rock to support itself—an important and not always understood fact.

C. Release of dangerous byproducts

1. *Radioactive materials.*—Gaseous and solid radioactive byproducts of the nuclear explosion are important because of the hazard they present to man and his environment. These radioactive materials are the result of fission-product formation or induced radiation in natural constituents of the media surrounding the explosion. Solid radioactive material may be subject to leaching subsequent to the detonation by circulating ground waters and solutions and could contaminate potable water supplies or cause the abandonment of natural mineral resources. Radioactive gases may be released to the atmosphere through venting of the explosion, with possible harmful effects upon the general public; or, upon reentry, these gases may result in overexposure of operating personnel. Miners may also be exposed to dangers from radioactive solids, dusts, or solutions in proximity to the shot point. Surface personnel in mills and refineries could be exposed to radiation hazards if the ore and rock removed from the blast area are contaminated.

2. *Combustible and toxic gases.*—Through the interaction of the nuclear explosion with rock constituents, highly combustible and possibly toxic gases may be formed. These gases could cause secondary detonations and create hazards on the surface if exceptionally violent reactions occurred, and could be particularly dangerous to persons attempting to reenter the shot area after the nuclear explosion.

3. *Means of control.*—Data will need to be developed for different rock types on the probability and means of insuring complete containment of radioactivity; the anticipated extent and configuration of the contaminated zone; properties of specific combustible and toxic gases and radioactive substances likely to be created; methods of disposing of these hazardous products; and techniques for monitoring and protection against radioactivity in underground operations which will meet Federal codes of regulation.

Question. You say that the long experience of mining industry should be utilized to the fullest extent. Isn't this being done at present? If not, why not?

Answer. In order to utilize the experience of the mining industry, representatives of the industry should be consulted at the planning stage of any project. Probably the most effective mechanism to accomplish this is an advisory committee at the so-called working level. In other words such an advisory committee should be composed of outstanding engineers and geologists, but not necessarily the busy top executives of the mining companies. Such a committee could also include geologists from the Geological Survey and mining engineers from the Bureau of Mines.

A committee to advise on the plowshare program was appointed in November 1959, but it is a "high level" committee, presumably to advise on policy. The mining industry is represented by one member, Dr. Donald H. McLaughlin, president of the Homestake Mining Co. Perhaps a subcommittee on mining applications could be attached to the policy committee.

Question. Can you explain what is meant by the second part of the recovery of oils from shale—the combustion?

Answer. To illustrate what is meant by the "second part" of an experiment to recover oil from oil shale by in situ combustion, let me quote more fully from Mr. Murphy's paper:

"Much publicity has been given to the nuclear phase of the proposed Plowshare experiment in oil shale, but many people still are unaware that there are two parts to this project, the second being a study of methods for producing oil

by in situ combustion of the broken shale after the explosion. This is the more important part and the principal justification for the whole experiment. However, it is more difficult to discuss than the nuclear phase, because plans must remain nebulous until results of the explosion are known.

"In situ combustion projects have been conducted with considerable success in several petroleum reservoirs. This may lead to the assumption that the same techniques can be applied to oil shale and that problems involved in doing so will be minor. Actually, the mechanisms and problems involved in recovering petroleum by this technique differ substantially from those we can expect to encounter in oil shale.

"In situ combustion in petroleum reservoirs involves igniting the oil in a porous reservoir rock and driving a combustion zone through the reservoir toward producing wells with compressed air, with which recycle gas sometimes is blended. Combustion produces hot gases which force the oil and water in the reservoir to producing wells.

"As the burning front moves slowly from the air injection well to the production well, several oil displacement mechanisms operate, more or less simultaneously. Steam produced from combustion and from water in the formation produces a condensing steam drive ahead of the front. Heat from the steam reduces the viscosity of the oil and promotes displacement. Gaseous combustion products heat the oil and increase its mobility further by dissolving in it, allowing the remaining combustion gases to drive it ahead more readily. Mild cracking produces light hydrocarbons which blend with the oil to produce a miscible drive. Thermal energy also is transferred to the oil by conduction through the sand ahead of the burning front.

"To control the process, a uniform and reasonablly symmetrical burning front must be maintained. Variations in permeability and oil saturation tend to cause a ragged front and the formation of liquid blocks may extinguish combustion at some points. The front is controlled by regulating the volume and pressure of injected gas and manipulating back pressure on the producing wells.

"No data have been published on in situ recovery experiments in Green River oil shale although one company reportedly has conducted such a test on its property. However, some problems that will be involved can be assessed by examining the composition and properties of oil shale and its behavior in other thermal systems.

"Green River oil shale is a magnesium marlstone with a finely laminated structure consisting of an intimate mixture of organic and inorganic materials.

"The mineral portion, ranging from 92 to 60 weight percent in shales assaying from 10 to 75 gallons of oil a ton, consists largely of dolomite, calcite, feldspar, quartz, and illite clay. Particles are essentially nonspherical and extremely small, over 90 percent having equivalent spherical diameters of less than 44 microns. In the leaner shales cementation is quite strong but decreases as the organic content increases until, in rich strata, it appears that the organic material is the principle cementing agent. The angularity displayed by these particles persists throughout all size ranges of all grades of Green River shale that have been studied. The weak cementation of these abrasive particles in average shale suggests that the burned shale may disintegrate and some of these particles be carried along with the combustion gases to plug the passageways, erode equipment, and contaminate products. Development of techniques to avoid this difficulty will be an important phase of the experiment."

Mr. Murphy discusses many other problems; those who are interested are referred to his paper.

Question. What is meant by block caving?

Answer. Block caving is a system of underground mining used to extract large tonnages of ore which are too far below surface to permit economical open-pit operations. Essentially the system involves undercutting a "block" of ore perhaps 100 feet by 200 feet in area. Once its support of underlying rock is removed, the "block" crumbles and caves. The weight of overlying ore and rock induces a continual caving action as long as the broken ore is removed from below. Haulage ways are maintained some distance below the "undercut level" and, as the ore caves, it is drawn through chutes into cars to be hauled to the shaft and hoisted to the surface. It is an economical mining method because, after the first undercutting, the breaking of the ore is accomplished by the force of gravity.

Question. What sort of experimental explosion condition could tell the most about mining problems? Is this type of experiment being planned?

Answer. I do not believe that any one experimental explosion, by itself, would give a complete set of answers to the questions concerning the feasibility of using

nuclear explosives for mining purposes. Rather, it would seem necessary to conduct long-range, basic investigations of nuclear explosives to ascertain the technical and economic feasibility of applying these devices to the development of our mineral resources. This research should not be undertaken with the intention of demonstrating to industry a proven tool that will economically facilitate the production of petroleum and minerals. Such cannot be the case in our present state of knowledge. A series of studies, laboratory investigations, and field tests should be undertaken to determine more precisely: (1) Industrial operations where nuclear devices may be utilized; (2) the characteristics of nuclear explosives which will affect their applications to mining; and (3) explosion phenomenology in various rock types.

Only after these investigations have provided a good understanding of the fundamentals can we realistically propose development projects to determine the feasibility of recovery of oil, copper, iron, and other commodities with nuclear explosives.

Question. Has much thought been given to stockpiling certain mineral resources obtained by this proposed technique? Would this affect the immediacy of completing the experimental work presently envisaged?

Answer. I have read that the strategic stockpiles of most of the mineral commodities are full. In most cases purchases have been terminated. If additional tonnages of metals or minerals were needed for stockpiles, they could be supplied by present productive capacity.

Representative PRICE. Due to the fact that Dr. Teller wants to take an early plane home, we have rearranged our schedule somewhat.

The next witness will be Dr. Edward Teller, director of the Lawrence Radiation Laboratory, at Berkeley.

Dr. Teller, we are glad to have you before the committee, as we are always glad to have you, to have your counsel and advice.

STATEMENT OF EDWARD TELLER,[1] DIRECTOR OF THE LIVERMORE BRANCH OF LAWRENCE RADIATION LABORATORY

Dr. TELLER. Mr. Chairman, members of the committee, it is a great honor and a very important opportunity for me to talk to you about a subject which many of my associates, and very specifically I, consider as a subject of the utmost importance.

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Naturalized: Mar. 4, 1941, in Washington, D.C.
Family: Wife, married in 1934 to Augusta Harkanyi; children, Paul and Susan Wendi.
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Education: Karlsruhe Technical Institute, Germany, 1926–28; University of Munich, 1928–29; Leipzig, Ph. D., 1930.
Positions: Research associate, Leipzig, 1929–31; research associate, Gottingen, 1931–33; Rockefeller fellow, Copenhagen, 1934 (with Niels Bohr); lecturer, University of London, 1934–35; professor of physics, George Washington University, Washington, D.C., 1935–41; professor of physics, Columbia University, 1941–42; physicist, Manhattan Engineer District, 1942–46 (1942–43, University of Chicago, 1943–46, Las Alamos Scientific Laboratory); professor of physics, University of Chicago, 1946–52; Los Alamos (on leave, University of Chicago), 1949–52; consultant, assistant director, Livermore branch, University of California Lawrence Radiation Laboratory, 1952–53; professor of physics, University of California, 1953–; associate director, Lawrence Radiation Laboratory, University of California, 1954–; director, Lawrence Radiation Laboratory, University of California, Livermore, 1958–.
Research: Dr. Teller's research interests are broad, and he has made important contributions in several fields—in chemical physics, molecular physics, nuclear physics and quantum theory. He was an early researcher in studies of thermonuclear reactions, the processes by which stars like the sun generate energy. In recent years he has attracted attention for his role in the practical application of thermonuclear principles in the development of thermonuclear weapons. He has made contributions to the spectroscopy of polyatomic molecules and in recent years has maintained an interest in the theory of atomic nucleus.
Honors: Doctor of science, Yale University, 1954; University of Alaska, 1959; Fordham University, 1960.
Awards: Joseph Priestley Memorial Award, Dickinson College, Carlisle, Pa., 1957; Albert Einstein Award, 1958; General Donovan Memorial Award, 1959.
Memberships: National Academy of Sciences; American Nuclear Society fellow, American Physical Society; American Academy of Arts and Sciences; Scientific Advisory Board, Air Force; General Advisory Committee, Atomic Energy Commission; American Ordnance Association.

Nuclear explosions are big events. They have caused in the past radioactive deposits. All of this gives reason for caution. One can carry caution so far as not to proceed at all. I believe that a continuation of this excessive caution is certainly not to be advised. You will hear a little later from Dr. Johnson, from our laboratory in Livermore.

Dr. Johnson will tell you much about a very well-thought through, immediate program and about the practicability of this program. It is important to select our first objectives so that they should be meaningful. In a new field like this, and in a big field like this, we have to do as much as possible by theory, and we have done a lot by theory. But the time comes when you have to experiment, and then you have to select the most fruitful experiments.

Talking a little bit ahead of schedule, I would like to do something about summarizing for you the general field. I would like to say a word about the applications where at present we cannot be sure of success and also about the applications where I think positive statements can be made about feasibility.

The uncertain area is the bigger one. That is in the nature of things.

Applications to energy production, applications on improvement of water supply, production of oil, chemical applications, are full of great and important uncertainties. This has been brought out, for instance, by questions of Mr. Hosmer. I believe that it is very probable that in some of these fields there will be great possibilities, but if we are asked at present about these fields what will be the result, I think that the proof of the pudding is still not before you. We have hope enough to justify cautious experimentations.

Let us go on to the proven applications. We know how to dig a hole. We know it because we have made quite a few hole-digging experiments. These hole-digging experiments show that we can make canals, that we can make harbors. We also know that we can strip off the overburden from rich ore supplies, and thereby increase our supply of raw materials.

I believe that these applications can be called certain. They can be carried out. In many cases they can be carried out very economically. But there is one really disturbing thing about them. In all these assured applications, some radioactivity will be injected into the atmosphere by our present tools.

There has been exceedingly great concern about radioactivity. The Joint Congressional Committee has done most admirable work in dispelling unfounded fears. Radioactivity is something about which we have to continue to be careful. We have to be careful not only in the surface applications, but we have to be careful in the underground applications as Senator Anderson pointed out a few minutes ago. We are trying to be careful about it, and we have made a lot of investigations, both on the actual site of the Nevada explosions, and in the laboratory. The problem is complex, and we will have to proceed step by step.

I would like to say a word of general reassurance, referring to a fact with which you are all familiar. One reason why strontium

was so disturbing is the following: Strontium gets down on the surface of the earth. Rain falls and the strontium is washed into the earth. More rain falls and the strontium gets deeper and deeper. You would hope to get rid of the strontium when it is washed down into the depth of the earth. But this does not happen. The strontium travels very slowly so that in practice it will remain at root depth, within a few inches of the surface. That is why it remains a noticeable factor at all.

Now, if we deposit these radioactive substances deep underground, let us say at a depth of 1,000 feet, then at least some of these substances will behave like strontium, and will remain in the immediate neighborhood of the original explosion: This is what we have found for the majority of the activities. It is not true for all activities. One has to conduct a case-by-case examination, and we have to continue to be extremely careful. I can give you every assurance that we are very careful and that we will not contaminate water supplies in a lighthearted manner or otherwise. We will just not do it.

At this point I should like to come to one of the crucial questions. There is a way of improving the situation. We can improve the whole situation by improving our tools; by new inventions. We can make our nuclear explosives cheaper, more flexible, more clean. There is no question about it in my mind that very great progress can be made. We know that today because of the excellent work of some of my younger and more vigorous associates. I know that this progress is possible. But it is a field where progress can be made only with continued and careful experimentation. We can theorize, but we have to prove it. One proof is the steppingstone to the next theory. I am sure today that we can make nuclear explosives much cheaper, more flexible, and much cleaner. I can say, not with certainty, but with quite a bit of hope, that we can make nuclear explosives for peaceful purposes so clean that the worry about radioactivity in its peaceful applications may disappear completely.

In performing these experiments, we need plans, which we have, and means—and I must say that we have been reasonably generously supplied with the means so far. I say reasonably generously only because from the point of view of a laboratory director one can never say more than reasonably generously. But there has been a serious question about experimentation at the present political stage of affairs. I feel that these experiments are very necessary, and I would like to urge that these experiments be encouraged in every possible way, because only in this way can Plowshare come to its fullest fruition. It can be very successful with the help of the tools which we now possess, but with future tools it can become really phenomenal.

I only wish that I could describe to you some of our proposed tools which, however, are in the secret category. It would be of great help to us if these experiments could be conducted with as much openness, with as little restriction and classification as ever possible.

It is wonderful to see that the Atomic Energy Commission has declassified everything about the action of nuclear explosives in Plowshare. It is wonderful that we are planning to proceed with a very important shot in New Mexico. We plan to proceed with interna-

tional cooperation and under international observation. All this is fine.

Our tools, the explosive itself, is, so far, classified. It would be unjustified in my opinion to declassify the explosive without careful investigation of what effect that has upon our national security. This investigation, itself, must necessarily be conducted in secret. However, I can say here one or two general things.

The tools which we want to develop are not military explosives. They are peaceful explosives. They are related to military explosives like a passenger car is related to a tank. The principles, or some of the principles, are similar. The execution is quite different, and the technical knowledge obtained from the one is only indirectly relevant to the other. The specific reasons for the differences are these:

A weapon must be light, easily deliverable. It can, on the other hand, be moderately expensive, because the overwhelming importance of the specific job that it performs justifies the expense. A peaceful nuclear explosive can be clumsy, big, heavy, and still it is an excellent experimental tool. It will be very much lighter and very much more flexible than the equivalent amount of TNT or of chemical explosive. At the same time, cheapness, flexibility, cleanliness are of utmost importance. Therefore, we seek to accomplish quite different aims. We accomplish them along different roads, and the two approaches, while related, are really quite distant from each other.

I therefore urge very careful investigation of the degree to which certain tools to be used in Plowshare can be declassified. While I feel that we should proceed immediately and without waiting for declassification. I should say that for the future, this question should be investigated.

I would now like to come at the end, to a hearty welcome to the statements that Professor Smyth has given in the introduction. He has raised basic points about Plowshare, some of which already have been discussed by others. I, for instance, feel quite sure that in a control system where underground explosions can be hidden so easily, nothing can be done without mutual confidence. And where should you have confidence if not in the works of peace.

At the same time, I am more interested in another aspect which I should like to draw to your attention. I have seen on many occasions people who have approached this problem of Plowshare, of the peaceful use of nuclear explosives. In spite of the fact that chemical, conventional explosives have been used peacefully, the idea of using nuclear explosives appalls people at first sight. Their use for peaceful purposes appears absurd, and most people feel intuitively that these uses will not pay off. The more our scientific associates and acquaintances looked at it, the more feasible many of these projects have appeared. You have heard some of the applications. You will still hear of others. But here I want to summarize for you quite briefly the situation.

I am convinced that nuclear explosives will pay off in extremely important fields, and will pay off at an early date if we are given the go-ahead signal. It will uncover and multiply our raw-material

supplies. It will help with our water problem. It will help with our problem of oil. I should not say "will," but I should say "may" in some of these important cases. It may help in many other ways. It surely will help in problems of transportation, in the building harbors and canals.

About these questions of raw materials, oil, water, energy, accessibility, wars have been fought in the past. Now we can bring these extremely important things to people who need them, We can build the sinews of peace if we have the courage and if we have the initiative. We can do so carefully, and we can do so safely. I believe that if we get the full support of our Government, many of the people with whom we are associated, the Government laboratories, Los Alamos, Livermore, the industrial companies, and many others will cooperate to explore this particular frontier. And if it is explored, I confidently predict that in the field Plowshare will become the first great economic success among the frontiers of peaceful atomic energy.

Thank you very much.

Representative PRICE. Thank you, Dr. Teller.

Senator Anderson.

Chairman ANDERSON. Doctor, you do want to declassify Plowshare so far as it can be reasonably done?

Dr. TELLER. Most definitely so.

Chairman ANDERSON. Do you think it would have good results internationally?

Dr. TELLER. I feel that if we can go to our friends abroad and tell them, "Here it is, what we are doing. You see the tools. You see the operations. You know how it works. We can tell you why we have confidence that it will work." I think that in this way it is much more likely that we will have their wholehearted acceptance, support, and their welcome.

Chairman ANDERSON. Thank you.

Representative PRICE. Mr. Van Zandt.

Representative VAN ZANDT. I have no questions.

Representative PRICE. Mr. Bates.

Representative BATES. Doctor, when would you hope to be able to attain your goal of achieving this clean explosion you referred to?

Dr. TELLER. I believe that the question of cleanliness needs quite a bit of experimentation. I have very good hopes that if we are told to go ahead with experimentation, then in 2 or 3 years from that date we will be able to come back with very substantial results, with cleaner and possibly with quite clean objects which we will know in detail how to produce and how to use.

Representative PRICE. Mr. Ramey.

Mr. RAMEY. In terms of your declassification, would that go to the extent of design? Would you be running a danger of putting other countries into the knowledge of how to make weapons? That is, the fifth and sixth country problem, so-called. It used to be the fourth country problem.

Dr. TELLER. Yes. I think this very mention of the numbers is significant. So far, every country which found out how to make the

nuclear explosive found out almost at once how to explode it. The secret of nuclear explosives is a far lesser secret than we hoped and than we believed. I think that we should not disclose our advanced concepts of nuclear explosives for wartime purposes. But the general principles cannot be kept secret. It is only these general principles which need become known. One could choose the design of peaceful explosives in such a way as to disclose a minimum of the information that is useful for bomb design.

The hints which we are giving that way I believe will not be very significant. They might even be, to some extent, misleading. I also believe that in disclosing the information we need not explain in detail how we perform our calculations, how we construct our theories. We merely say that the black box into which we put our nuclear explosive is no longer black. Here it is; we can open it. This part serves this purpose and this part serves that purpose. This is the sequence of events that occurs during an explosion.

We need not tell people: This is how we perform our difficult calculations in detail. If we make disclosures in a discriminating way, I think we can reassure everyone.

Had we been governed by utmost caution, then we would not have disclosed to the world nuclear reactors either. In that case, history would have passed right by us, and we would be now left behind. I think we should not be left behind in the race for the peaceful atom. The question of to what extent declassification can help us in Plowshare deserves very careful scrutiny.

Representative PRICE. Mr. Westland.

Representative WESTLAND. I have no questions.

Representative PRICE. Thank you very much, Dr. Teller. We hope you can make your plane.

Dr. TELLER. Thank you.

Representative PRICE. We appreciate your having come.

Dr. TELLER. Thank you.

Representative PRICE. The next and final witness at this afternoon's session will be Dr. Gerald W. Johnson, associate director of the Lawrence Radiation Laboratory.

Representative WESTLAND. Could I make a suggestion that Dr. Teller's summary be put into the record.

Representative PRICE. The summary will be inserted at this point.

(The summary referred to follows:)

SUMMARY OF PLOWSHARE PROGRAM BY DR. EDWARD TELLER

Nuclear explosives which have been developed to date are sufficient to prove the usefulness of these tools in works of peace to start our Plowshare program. But it is also important to consider further developments of nuclear explosives directed specifically toward peaceful purposes rather than the applications in war.

The differences between these two types of applications are great. In a weapon easy deliverability is a most important requirement. Within certain limits the cost is not decisive.

An instrument of peace which is to be used in an engineering enterprise may be quite clumsy without affecting its usefulness. At the same time low price in this case is of considerable importance. A low price is particularly interesting

when we are concerned with small nuclear explosions. When an earthmoving or rock-cracking job of extremely great dimensions is to be undertaken there is no doubt that on the basis of present technology nuclear explosives are less exepnsive than conventional tools of engineering. In case of small explosions of a few kilotons the usefulness of nuclear explosives depends more critically on their price. One must remember that in many cases small explosions might find more ready acceptance. One also should remember that in many earthmoving jobs a properly placed series of small explosions can do a better job than a single big explosion. For this reason of flexibility, as well as the economic reasons mentioned above, further development of nuclear explosives is of importance.

The most important progress to which we can look forward in peaceful applications is the perfection of the cleanest possible nuclear explosives. We need these clean explosives in all sizes and for very many jobs. Popular apprehension of radioactive fallout has assumed proportions which may raise difficulties in any applications of nuclear explosions. Much has been done by the careful investigation of the Joint Congressional Committee which has served to allay unfounded fears and which has contradicted unjustified exaggerations. The fact is that we know how to handle nuclear explosives with complete safety. Nevertheless Plowshare will be even more readily accepted and more easily and cheaply applied if radioactivity can be greatly reduced or perhaps completely eliminated.

There are known to us at present several methods by which cleanliness and flexibility can be improved and by which the price of nuclear explosives can be decreased. But in order to make real progress along these lines the experimental approach must not be neglected. For this purpose appropriate experiments with nuclear explosives should be undertaken. The tools which these experiments develop will differ from nuclear bombs at least as greatly as a passenger car differs from a tank. Certain general principles are similar, but the purposes are different and so are the tools of execution.

The Atomic Energy Commission has proceeded with a general and well conceived program to make the effects of nuclear explosives available for peaceful purposes without unnecessary restrictions of classification. According to the present procedures, however, free discussion can extend only to the effects of nuclear explosions and must rigorously exclude the nuclear explosive itself. It is natural and proper that any discussion of the nuclear explosive be scrutinized most carefully and that the question, to what extent public discussion of the explosive can affect national safety, must receive first consideration.

At the same time the differences between the Plowshare devices and the nuclear bombs are so great as to justify a new look at the classification of the Plowshare devices. This consideration itself must obviously be carried out in secret. In an open discussion we can merely point out the possibility of such declassification and also its usefulness. It is possible to carry out the needed developments of nuclear explosives under the present security regulations. These developments are urgent and I should urge that we go ahead with all possible speed and that we do not wait for any modification of our present rules. But in the long run it is obvious that a fully disclosed Plowshare device will find more ready and wholehearted acceptance throughout the world. It is also obvious that declassification will make it easier to separate the problem of Plowshare from any question of disarmament.

It is important to reemphasize that our present nuclear explosives are cheap enough and safe enough for immediate application to perform most important jobs. However, further developments of our tools could be invaluable. Such developments should not be unduly hampered by secrecy.

It is my conviction that if we take appropriate initiative the peaceful use of nuclear explosives may become the first truly economic big scale use of nuclear energy for the benefit of mankind. If the development of new tools can be added to our old instruments then I believe that among the frontiers of nuclear energy Plowshare will become the most successful as well as the most advanced.

Representative PRICE. Dr. Johnson, you may proceed with your statement.

STATEMENT OF GERALD W. JOHNSON,[1] LAWRENCE RADIATION LABORATORY

Dr. JOHNSON. Thank you.

Mr. Chairman and members of the committee, it is an honor to have this opportunity to discuss with you some of the potentialities of the Plowshare program. I shall cover the use of nuclear explosives in the areas of excavation, power, and water resources.

The planned experiments have a dual purpose, designed first to develop technical information concerning the predictions and theories associated with the experiment; secondly, and I think this is extremely important, they will serve as demonstrations of the kinds of things that can be done and, therefore, will set the psychological stage for additional steps.

I. EXCAVATION

Of all the applications that have so far been considered, the one we are most certain of accomplishing with very large economic gains is in earth moving. Some areas in which the excavating potentialities can be exploited are the construction of harbors, canals, and other waterways, the removal of overburden in some special situations to permit open-pit mining of ore, the construction of reservoirs for water supply or flood control, and the damming of rivers or streams to provide for water storage, which would also provide recreational lakes.

The economic advantages seem clear, as will be indicated by a few examples. There are some technical difficulties, but the experimental program required to solve them has been apparent for some time. The major problem arises from the radioactivity released by the explosives, but here also something is known about how to control it and what program course is needed to resolve this question. There can be no doubt that these programs will be successful.

Let us consider the question of the economics, present and projected. The AEC published, for the purpose of illustration and guidance, estimates of the likely charges for nuclear explosives in several

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[1] Born in Spangle, Wash., and attended elementary and high school there. Subsequently, he attended Washington State College—B.S. 1937, M.S. 1939 in physics, and the University of California at Berkeley, where he obtained a Ph. D. in physics in 1947. In the period 1941–46 he served in the Navy at the Naval Proving Ground in Dahlgren, Va., where he carried out experimental development work in armor and armor-piercing projectiles.

At the end of the war he returned to University of California to complete his graduate work and then he became an assistant professor of physics at Washington State, where he taught until 1949.

From 1949 to 1951, he carried on research on crystal growth and neutron physics at the AEC's Brookhaven National Laboratory in Upton, N.Y. In 1951 he went to Washington and worked in the nuclear weapons effects field at the Armed Forces special weapons project. After serving for a short time as a special assistant to the Director of Research of the AEC in 1953, he joined the Physics Department at UCRL. He was in charge of nuclear test operations for the laboratory for 5 years. During that period he participated in four weapons test programs both in the Pacific and in Nevada. Dr. Johnson was the Test Director for the Nevada test organization for the last two weapons series in Nevada. In this capacity he was responsible for the scientific phase of the operations.

He has participated in the investigations of peaceful applications of nuclear explosives, known as the Plowshare program, since its inception. As associate director of Lawrence Radiation Laboratory he now guides the Plowshare technical program as well as test activities. Demonstration experiments under consideration include the large-scale excavation experiment in Alaska, recovery of oil from Colorado oil shales, recovery of oil from tar sands in Alberta, and an experiment in Carlsbad, N. Mex., to investigate the possibility of utilizing the heat of an atomic explosion for power or process heat and the neutrons to make isotopes.

energy ranges. The statement as issued by the AEC in September 1958, is as follows:

1. The charge for fabricating and firing a device 30 inches in diameter and of a few kiloton yield, all from fission, would approximate $500,000 when made available in small numbers.
2. The charge for fabricating and firing a device 30 inches in diameter of a few 10's of a kiloton yield, all from fission, would approximate $750,000 when made available in small numbers.
3. The charge for fabricating and firing a device 60 inches in diameter in the yield range up to 5 megatons, of which 5 percent of the yield was from fission and 95 percent from fusion, would be approximately $1 million in small quantities.
4. In the event of multiple firing in the same location, or in using large numbers of devices, the charge for firing would be substantially reduced.
5. These charges are only those incident to the fabrication of the device, emplacing it in its firing location, making the firing attachments, firing, and studies to assure public safety and to determine the results of the detonation. It does not involve such possible activities as preparing a hole or other structure for the firing or studies to determine the results of industrial utility.

The crater dimensions to be expected for large nuclear explosions, today, must be estimated from quite limited experience. The U.S. knowledge, with the exception of a small though important program, has been derived from experiments for military purposes. It is only in the last year that the Plowshare program has been able to support a small high-explosives program. The pertinent nuclear experiments are listed in the following table.

(The table referred to follows:)

Table I

Event	Date	Yield (kilotons)	Depth of burial (feet)	Crater dimensions			
				Diameter in feet	Depth in feet	Volume (cubic yards)	Volume per kiloton (cubic yards)
Buster Jangle U	Nov. 19, 1951	1.2	17	254	53	37,000	31,000
Teapot S	Mar. 23, 1955	1.2	67	294	90	96,000	80,000
Neptune	Oct. 14, 1958	.09	100	200	35	22,000	240,000

Dr. JOHNSON. Because large scale projects would require explosions in the hundreds of kiloton, or megaton range, it is necessary to calculate from the data in table I the cratering capabilities of these much larger explosions. To do this, a theory was developed, followed by a check of the theory against the nuclear explosions in table I and cratering data from chemical explosions.

In the last year a series of 256-pound chemical charges were set off in desert alluvium to get the effect of depth of burial on size of crater produced. Included in the program are three explosions of 20 tons each, to be fired this spring to get an indication of the scaling laws—which will tell us how to calculate crater dimensions to be expected for various charge weights. It is from this information that expected crater dimensions for large nuclear explosions are calculated. As is obvious from the very limited experience we have had, our projections cannot be expected to be too accurate.

The following table (table II) has been developed to show what to expect in crater dimensions for nuclear explosions placed at such depths that maximum crater dimensions result.

(The table referred to follows:)

Table II

Yield	Diameter in feet	Depth in feet	Volume (cubic yards)	AEC charges	Construction and operational cost	Cost per cubic yard
1 kiloton	400	100	230,000	$500,000	$1,000,000	$6.50
10 kilotons	790	195	1,200,000	750,000	1,500,000	1.90
100 kilotons	1,550	385	13,000,000	1,000,000	2,000,000	.23
1,000 kilotons	3,000	760	95,000,000	1,000,000	2,500,000	.04

Dr. JOHNSON. The costs per cubic yard listed in column 7 are to be compared with conventional costs of $2 per cubic yard. The excavation costs by nuclear methods listed in table II are based on published AEC charges. If one desires to look ahead 5 years with active development to reduce costs of explosives, the AEC charges could be reduced by several fold. In addition, with the development of cleaner explosives and with further experience, the operational costs could probably be cut in half (1960 dollars and support costs). With an aggressive program for the development of explosives designed especially for the Plowshare program one can anticipate in a relatively short time that even at 1,000 tons, excavation costs with nuclear explosives will be much less than by conventional methods.

At the million-ton level the overall costs might approach only a fraction of the costs listed in table II. One implication of these projections is that consideration can be given to accomplishment of projects hundreds of times larger than were heretofore possible to seriously consider.

Representative PRICE. What would be the relative time element difference between the construction of a sea-level canal across Panama, say, with nuclear explosions as against the conventional digging of the same canal?

Dr. JOHNSON. We have done a paper analysis of this. We estimate that assuming we have the experience, so we know where the proper placements might be and so on, which we think would take about 5 years to get, we believe the excavation of a sea-level canal could be accomplished in 2 years with nuclear explosives. This is taking into account all the safety factors, the maximum explosives we can use and so on.

By conventional methods, I do not know. The estimates that we have are more like 10 years in construction.

Representative PRICE. I do not want to get confused. We are talking about excavation at one point and construction at another.

Dr. JOHNSON. I am sorry. I am talking about excavation. To get a finished canal after the nuclear excavation, it would take about an additional 2 years. So from the time one started to dig to the time one had an operating canal with port facilities and all those things, it would take more like 4 years with nuclear methods.

With conventional methods it would be in the 10- to 12-year range. Those are the kind of comparisons we have come up with. Does that answer the question?

Representative PRICE. Yes.

Dr. JOHNSON. It is important to point out that the projected costs per cubic yard in table II are based on use of the total volume of the

crater, which is fair if the entire volume is used, as would be the case for water storage, flood control, or overburden removal. For construction of harbors or canals it is only the first 60 feet of depth that is necessary so the cost comparisons are not as favorable as the table would suggest for such construction because to obtain the required depth the excavation with nuclear methods must be deeper than would be done by mechanical methods; however, in specific examples so far studied for large projects even in these cases the nuclear methods would be severalfold cheaper.

The program needed to develop and demonstrate the nuclear method of excavation is outlined as follows:

1. Chemical explosives cratering program

The purpose is to develop scaling laws, a theory of cratering, and the dependence of crater dimensions on depth of burial and nature of the medium; to determine the placement of multiple charges to dig canals and harbors; and to learn the effects of collapsing canyon walls to form earth dams.

The chemical explosions would mostly involve small charges up to tens of tons, but should include a few in the nuclear energy range (thousands of tons) to permit crossover in calculation to nuclear explosions.

2. Nuclear explosive cratering program

Several nuclear explosions will be required in the kiloton range at various depths and in different kinds of rock and soil. The purpose of these small explosions would be to tie nuclear cratering results to chemical explosive results, and to obtain data on the effectiveness of containment and localization of radioactivity. The Neptune event (90 tons at depth of 100 feet) indicated that large craters can be produced under conditions such that 99 percent of the radioactivity is trapped deep underground and the remainder is deposited locally on the surface.

This is an important point from an operational standpoint and also from the general safety standpoint.

A most important part of the program involves large-scale nuclear experiments to confirm laws developed from data obtained at small scales and to demonstrate what can be accomplished.

The first of these projects is completely designed and ready to go to construction. The project, named Chariot, is planned for the northwest coast of Alaska and involves the firing of four 20-kiloton explosions in a line with one 200-kiloton explosion at the end. From this single experiment, scaling laws up to 200 kilotons will be obtained, data on containment of radioactivity and its localization will be available, the canal producing capability of nuclear explosions will be obtained, and there will be a demonstration at full scale. The earliest date at which this experiment could now be conducted is spring of 1962.

In the Nevada explosion of 100 tons, it was at a depth to give maximum excavation and in that explosion 99 percent of the activity was underground. If one used cleaner explosives, then one gets a cascade of the containment. If you have an explosive that has 5 percent fission energy and fire it under these conditions, then five-hundredths percent of activity gets out in terms of the energy release.

Our experience in the nuclear range is limited to 1 kiloton. In

order to do the kind of projects we are thinking about, we have to have information in the tens of kilotons and hundreds of kiloton range. To this end we have designed an experiment to be conducted on the north coast of Alaska. The purpose is to get the technical information we need on the craters to be produced by large explosions, when the nuclear explosion is placed at such a depth, to give maximum dimensions, and also to check our calculations on containment of radioactivity in large explosions.

A third point is to check the effective line charge work to cut canals. We propose to fire four 20-kiloton shots and one 200-kiloton shot to form a canal and a basin at the end.

The question of radioactivity control is of the utmost importance, but a great deal is known about it. In the case of use of nuclear explosives for excavation purposes confirmatory data are required from experiments, but based on what we know now, forecasts can be made with some confidence.

For the 100-ton Neptune nuclear explosion several important effects were observed.

(*a*) One to two percent of the gross radioactivity escaped from underground. There is a preference for the Sr^{90} and Cs^{137} to escape because they are decay products of rare gases, so that 3 to 5 percent of these isotopes appeared on the surface. The general local contamination levels were lower by at least tenfold over a surface explosion.

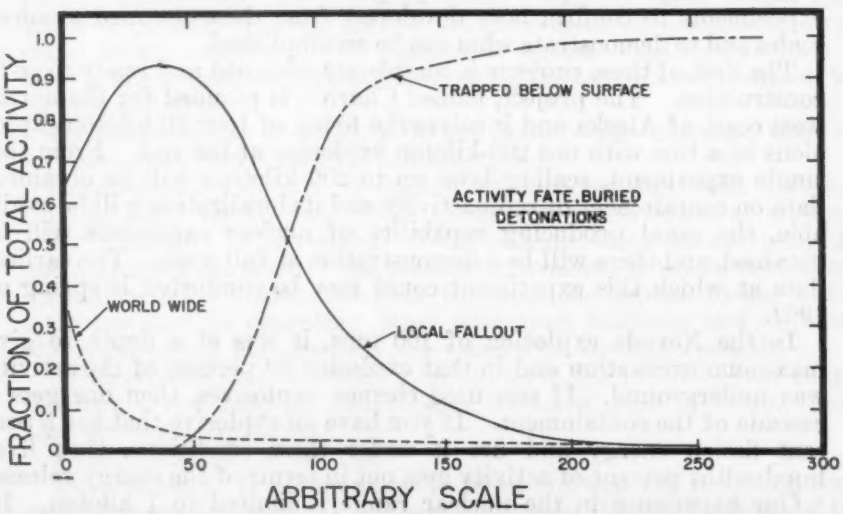
(*b*) The radioactivity that is released to the atmosphere should return quickly to the earth's surface because of the scavenging action by large particles thrown up with the explosion.

(*c*) The cloud rose only to a height of 1,000 feet, so that only low-level local meteorological conditions affect the results, and resulting fallout can be readily predicted.

(*d*) The radiation level in the worldwide contribution to fallout was below detectable levels 50 miles from Neptune.

The general effects of containment of radioactivity as it depends on depth of burial of the charge is shown in figure 1.

FIGURE 1



Plot of radioactivity containment as a function of depth of burial of charge.

Dr. JOHNSON. The important economic questions that must be answered are how soon after the explosion and in what areas may operations and living be resumed. Immediately after detonation, using presently available explosives, a limited area, which decreases rapidly with time, will need to be controlled.

Assuming that large explosions at appropriate depths will behave similarly to the 100-ton explosion and knowing that large explosives are available with 5-percent fission, we make the following estimate for a 10-million-ton explosion.

(*a*) At time of detonation—control zone must extend approximately 20 miles downwind and in a circle around the detonation point of 5-mile radius.

(*b*) At the end of 2 days the zone outside of which people would receive less than 5 roentgen lifetime dose would be 10 miles long and in a circle of 2 miles radius around zero.

(*c*) By the end of 3 months the entire area is below this level. Operations in the crater area could start on a 40-hour week basis, or shorter term work could be resumed earlier.

It is recognized that this extension of data from 100 to 10 million tons is very large indeed, so the specific numbers cannot be considered to be well known. Many other details in the radioactivity are also unknown. A major contribution to the understanding of these phenomena will be derived from the Chariot experiment.

3. Nuclear explosives development program

We know that the present nuclear explosives can be improved by large factors, given authority and support to develop them. The developments would lead to systems with very low amounts of associated radioactivity, and would be very much cheaper than the published charges. In addition, flexibility in design would permit construction of explosive systems to meet in the best way special problems, as, for example, the recovery of Athabasca oil, where there will be a tradeoff in cost of drilling and diameter of the explosive package.

Reducing radioactivity will simplify operations, in cratering particularly, by large factors. This successful development will not only multiply the number of tasks that could be undertaken, but because of general reduction of operational problems, would lower costs.

For greatest rate of advance, all three phases of the excavation program should be supported and encouraged at a level commensurate with the enthusiasm and competence of the individuals interested in attacking this problem. With this support, major industrial and governmental projects could be underway in a few years.

Representative PRICE. Could you give us something on the Project Gnome?

Dr. JOHNSON. Yes. First, I would like to say something about the Russian high-explosive experiments.

Incidentally, I do not subscribe to the point of view that just because the Soviet Union is doing a particular job we should tackle it with greater funds and manpower. However, I think it would be of interest to you to know what the Russian program on industrial use of explosives has been.

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As you will recall, on announcing the detonation of the first Soviet atomic bomb in 1949, Vishinsky stated the explosions were being used only for peaceful purposes, "to move mountains and irrigate deserts." While no such results of nuclear explosions have ever been published, the Soviet Union has followed a consistent program of using very large chemical explosions for such purposes. In late 1956, a paper entitled "On the Use of Nuclear Explosives for Industrial Purposes," by G. J. Pokrovsky, was published. In this publication chemical explosions up to 1.6 kilotons were described as being used for overburden removal in China. By the end of 1956 two more large explosions were set off—one of 4 kilotons and one of 9 kilotons. (See p. 69, for report on "Soviet Program for Industrial Applications of Explosions.")

To develop the theory further, a highly instrumented cratering explosion of 1,000 tons was fired in late 1957 near Tashkent. The theory and experiment had now been extended into the nuclear yield range. Attention then shifted to the study of firing rows of point charges for canal excavation purposes. First a series of smaller scale experiments were conducted which culminated in an explosion of 3,100 tons in 31 charges, presumably all of 100 tons each. This explosion produced a canal 100 meters wide, 25 meters deep, and 1,150 meters in length, and required no additional work. Plans were announced to fire 30 kilotons in a similar fashion by late 1959, but that has not yet been accomplished.

On March 5, 1960, the Russian press announced that—

a council for the use of explosives in the national economy has been set up under the Siberian branch of the U.S.S.R. Academy of Sciences. The Siberian scientists propose to strip a 45-meter-thick coal seam at the recently discovered South Yakutian coking coal deposit with the help of two big explosions—40,000 tons of ammonal each. The seam lies at depths of 40 to 60 meters under a layer of soft sand.

This tremendous explosion can also yield valuable information on the geological structure of this part of Siberia. The cost of explosion stripping will be several times smaller than the usual methods.

The data from these explosions would be useful to us in our Plowshare program. I think it would be useful to suggest an exchange of information with the Soviets. The data they have developed in these explosions, and they have been very careful to take advantage of them from a scientific standpoint, could make a major contribution to the peaceful uses of explosions, either chemical or nuclear.

Representative WESTLAND. These were not nuclear explosions?

Dr. JOHNSON. No, sir; they were chemical explosions, but they are up in the range where even with ammonium nitrate the costs of using nuclear explosions would be very much less for this kind of a job. They are certainly developing the technique so that the crossover to nuclear explosion techniques would be relatively simple.

To continue, the item states:

The scientists also propose to build a dam with the help of explosives in the Malaya Alma-Atinka River Gorge in Kazakhstan, a few kilometers above the Modeo Mountain Skating Rink. This dam will shield the republican capital of Alma-ata from the destructive mud and rock streams which rush down from the mountains approximately once every 50 years.

The dam is to be almost 100 meters high. It will be built by blasting the gorge sides, which will require the simultaneous detonation of approximately 5,000 tons of explosives. To raise the dam to the projected mark it is necessary to make two smaller explosions.

Explosion methods [the article from Vacherny Novosibirsk continues] open up great prospects for the deepening of riverbeds. The volume of these works on the Siberian rivers alone will amount to many hundreds of thousands of cubic meters in the current 7-year plan period, 1959–65.

The scientists have advanced a new theory of destroying the ice on waterways with the help of explosions. They have also developed a unit for drilling shot holes in hard rock with a large number of continuous blasts. A special fluid is used as an explosive.

It was recognized by the Russians in 1956 that at about 10 kilotons it becomes economic to use nuclear explosives in excavation and also that the radioactivity can be adequately controlled. They now have all the necessary data with high explosive experiments to proceed directly to nuclear projects, if they have not already done so.

Unfortunately the Soviet Union has not published results in sufficient technical detail to make it possible for us to use the results analytically, though from what has been released, we seem to be in essential agreement. The Soviet Union could make a concrete and substantial contribution to the U.S. Plowshare program by releasing details of all the results of their experiments. By this action the Plowshare excavation program could be advanced by at least 2 years.

Specific excavation projects that have been or are under study as part of the Plowshare program which could be initiated on completion of the experimental program outlined are as follows:

1. A small harbor near Katalla on the coast of the Gulf of Alaska to permit exploitation of possible coal and oil reserves. This harbor would be similar in size to the Chariot Harbor experiment.

2. Other harbors have been suggested along the west coast of South America, but specific studies have not been made.

3. A paper study has been made of the feasibility of constructing a sea level canal across the American Isthmus. The conclusion is that it can be done with a severalfold saving below the cheapest estimate, using any other method.

4. Although it has not been evaluated in detail, there are undoubtedly sites where small harbors could be developed to permit access of small boats to important recreational islands.

5. An Analysis has been made of the landslide created by the earthquake near Yellowstone Park last year which dammed the Madison River to produce a lake. It has been estimated that a 100-kiloton explosion placed at scaled depths similar to the 100-ton explosion in Nevada would produce the same amount of earth movement. The total cost to accomplish this program would be less than $5 million. The results from the projected 5-kiloton Russian explosions would be helpful in this analysis.

This list of possibilities is by no means complete, but it is illustrative of the kinds of undertakings that would be possible in a few years.

II. POWER

Let us now turn to the possibility of power production. Since in explosions we do know how to release thermonuclear energy on a large scale, we have the basic energy source available to us. Various ideas have been suggested for the conversion of this energy to recoverable heat. The simplest scheme is based on the observed result from underground explosions that about one-third of the energy release of the explosive is deposited in melted rock at high tempera-

ture. To this end various naturally occurring materials were analyzed, and it was concluded that salt offered perhaps the best possibility. With the simplest concept, nuclear explosions deep in salt domes would produce large pools of melted salt. The energy could then be recovered by driving some working fluid, like water, through it to produce hot gas or steam.

Theoretical calculations indicate that by multiple firings in the same location, as much as 50 percent of the total energy released could be recovered. Assuming a 30 percent efficiency of conversion of the recovered heat to electrical power, it is conceivable that fuel costs could be reduced to a fraction of 1 mill per kilowatt-hour. Using present published AEC charges, and assuming 1-million-ton explosions are used, the fuel cost is 6 mills per kilowatt-hour. This is to be compared with the average U.S. fuel cost of 2 mills per kilowatt-hour.

A better method, which would deposit energy directly in steam rather than first in the form of molten salt, seems more difficult technically. The plan would require the excavation of a large cavity underground filled with low temperature steam or partially filled with water. The detonation of an explosive of suitable size would lead to the direct creation of a reservoir of high-temperature high-pressure steam containing nearly all of the energy.

The size cavity required is larger than any so far built and would impose rather formidable practical construction difficulties; however, recent experiments in decoupling seismic signals in salt show that the cavity might be several times smaller than originally thought. In any case, this scheme merits further attention and study because of its potentially high efficiency.

Our program in this area includes high explosive and laboratory experiments, as well as considerable theoretical effort. In addition, one nuclear experiment, Gnome, is ready to go to construction. This experiment will be the first exploratory step in depositing the energy in salt and recovering it with a transfer fluid. In about 2 years, we could be ready to do a small scale experiment in a cavity.

Some consideration has gone into the use of nuclear explosions to release geothermal power. In principle, the idea is to capitalize on zones of the earth where massive high temperature rocks are near the surface. For example, if 400° C. rocks are available at depths of 15,000 feet, then an explosion could be used to shatter the formation. The energy could be recovered then by permitting water to come in contact with the hot rock.

It has been estimated that if heat is extracted in this way until the rock drops to 200° C., the recoverable energy would be several times the energy of the explosion. There are many uncertainties in this idea, but it is something to keep in mind as more experience is gained. No major experiments are planned in the immediate future, although some small scale measurements are being considered in the recent lava flow in Hawaii.

All in all, power generation seems very difficult technologically but it certainly is worth doing some exploratory work on because today it is the only way in which we have any direct evidence of the possibility of using thermonuclear power.

Representative PRICE. Mr. Ramey has a question.

Mr. RAMEY. I think this shows some progress, because most of the time on nuclear fission the projected cost always comes out as almost at or a little bit cheaper than the cost of conventional power. Of course, Admiral Rickover always said the way you will cheapen nuclear power is to cheapen the cost of coal. So you are several magnitudes below that.

Dr. JOHNSON. Yes; the reason is I am talking about thermonuclear power, because in the explosions we do release the fusion energy. I am making a projection, also, and I want to point that out.

If one uses 1-million-ton explosions, from the chart, $1 million, as I indicated, on that basis the full cost is 6 mills per kilowatt-hour. The fraction of the mill projection comes from a projection of what we think is realistic and possible with the development program.

Recently we have returned to an old idea with respect to power production. It is some 2 years old. The most efficient way to convert the energy to power is to convert the energy or deposit the energy directly into steam. Two years ago calculations were made covering the use of a large cavity underground containing water; firing the explosive in this environment would then produce steam which could be tapped as required for generation of power.

Mr. RAMEY. Would there be radioactivity inside the cavity that you mentioned a moment ago?

Dr. JOHNSON. There would be radioactivity, so it would be necessary to have a closed system to the turbine or isolating heat exchangers.

Representative WESTLAND. How long would the power last when you create it? What length of time would it last?

Dr. JOHNSON. To operate a powerplant at, let us say, 500 megawatts, and I would have to be sure about this, one would require a million-ton explosion every 10 to 15 days.

Representative WESTLAND. Would you say that again?

Dr. JOHNSON. To run a 500-megawatt powerplant, one would need to fire a million-ton explosion every 10 to 15 days. In the cavities, if they can be constructed, this could certainly be done. Alternatively, one would have to deposit the energy in a large field deep underground, as illustrated by the Gnome experiment, and recover it later. One could, in principle, deposit enough energy to operate the plant over its full life. But this would require setting off a series of explosions, accumulating the energy underground, and in essence mining it as required. However, we don't think that is the best way to do it and would prefer depositing the energy either chemically as suggested by Dr. Abelson, or in a large cavity.

Representative Westland. And you wind up, you think, with a fraction of a mill cost for the power?

Dr. JOHNSON. Yes, sir; that is what the calculation showed for the fuel cost.

III. WATER RESOURCES

In the field of water resources, the results from the cratering program can be applied either by direct cratering or by collapsing canyon walls to build earth dams.

Representative BATES. I do not understand the entrapment. Say you want to dig a canal 40 or 50 feet deep and you get the small charges lined up and have the explosion so all of this stuff goes up into the air. What happens?

Dr. JOHNSON. What happens is that by using deep placement, the material above the point of detonation is heaved and the top surface bounces off with the lower portion falling back in. The 100-ton explosion in Nevada was placed at a depth of 100 feet. After the explosion, the crater was 40 feet deep, so there was 60 feet of rubble between the bottom of the crater and the point of detonation. In drilling through this zone and examining it, it was quite clear that the material had just heaved and fallen back down with little mixing, but the material near the top was bounced off and displaced down the slope. The bulk of the radioactivity was trapped deep in the rubble zone. This is the general view of the mechanics of entrapment of radioactivity.

An entirely different kind of possibility was suggested by the 20-kiloton Blanca explosion in Nevada. For that explosion a rubble zone was created all the way to the surface but with a very small crater. The shot, buried at a depth of about 900 feet, shattered a zone from a hundred feet below the shotroom all the way to the surface. It has been estimated that a storage volume of about 40 million gallons was created directly and that a permeable link to the surface was produced. The cost of accomplishing such a storage volume in this way would be about $3 million.

This is a high cost for storage, but there are situations that we know about where this cost would not be considered to be excessive. In regions where evaporative losses would be large for surface reservoirs, the water would be stored underground and the evaporation loss would be reduced to zero. However, the idea is more attractive if one considers using the explosion to help nature. For example, where there are natural underground permeable zones either empty or being depleted, it would be possible to use the explosion to create a permeable zone and divert surface water into the natural reservoir.

There are many regions in which the removal of natural barriers to the flow of underground water or the development of appropriate sumps for accumulation of water would contribute to water resource development, storage, and distribution. In concept we should look ahead to the water needs of future generations, by storing a fraction of surface runoff now to accumulate it underground and recover it later by pumping.

The question of takeup and migration of radioactivity in water needs to be thoroughly investigated at each site prior to setting off an explosion. However, in general, from theoretical considerations, taking into account the behavior of underground explosions and including experience from the disposal of radioactive wastes, it appears that the availability of radioactivity to ground water can be controlled to acceptable levels.

Experimental programs must be conducted to measure and to develop detailed understanding of the behavior of radioactivity in various underground water situations. In addition, if it later seems desirable, a nuclear experiment could be conducted to permit the accumulation of practical experience and data on the conservation of water by these methods.

With an active and well-supported program, there can be no doubt that nuclear and thermonuclear explosions can become powerful and effective tools for excavation projects in the immediate future. The

simultaneous carrying out of an aggressive program for the development of special explosives will expand the applicability enormously and will reduce the costs substantially.

Power production from thermonuclear explosions poses formidable but not impossible technological problems. Because tremendous gains are possible, the power possibilities should be thoroughly explored, including special situations which might arise from extraordinary geological conditions.

Substantial contributions can be made to the fields of water conservation and flood control. What this program mostly needs now are detailed analyses by water resources and geological groups, followed by appropriate nuclear experiments.

Thank you for the opportunity to present our program. The United States has in its hands the most powerful tool ever invented; we ought to take the lead in developing it and using it for the benefit of mankind.

Representative PRICE. Thank you very much, Dr. Johnson, you have presented a fine statement.

Dr. JOHNSON. Thank you.

Representative PRICE. That concludes the witnesses for this afternoon.

The committee will stand in recess until tomorrow morning at 10 a.m., when we will meet in this same room. The first witness will be Dr. Paul McDaniel.

(Paper referred to on p. 62, dealing with Soviet Program for Industrial Applications of Explosions follows:)

UCRL-5932-T

THE SOVIET PROGRAM FOR INDUSTRIAL APPLICATIONS OF EXPLOSIONS

LAWRENCE RADIATION LABORATORY
LIVERMORE, CALIFORNIA

MARCH 28, 1960

THE SOVIET PROGRAM FOR INDUSTRIAL APPLICATIONS OF EXPLOSIONS

At the outset of its nuclear weapons development program and following its first successful tests in 1949, the Soviet Union recognized the importance of peaceful applications of nuclear explosives. In particular, the potential excavating capabilities of the explosions were obvious to them. As a consequence, in acknowledging the first successful test, Mr. Vishinsky made this statement to the United Nations in November of 1949,

> "The Soviet Union is now using atomic energy for peaceful purposes, not to stockpile bombs, but if, unfortunately and to our great regret, this were necessary, we should have as many of these as we should need -- no more and no less. Right now we are utilizing atomic energy for our economic needs in our own economic interest. We are raising mountains; we are irrigating deserts; we are cutting through the jungle and the tundra; we are spreading life happiness, prosperity and welfare in places wherein the human footsteps have not been seen for a thousand years."

In 1956 a paper entitled, "On the Use of Nuclear Explosives for Industrial Purposes", by G. I. Pokrovskiy was published in Russia.[1] The paper is an analysis of the potentialities of nuclear explosions for excavation and is directed primarily toward overburden removal in the development of open pit mines. Included is a discussion of the radioactivity produced and its distribution, which is followed by the conclusion that radioactivity "should not be considered an insurmountable obstacle to the use of such explosions in mining and construction". The final statement in the report is, "On the basis of the many advantages of nuclear explosions we conclude that the time is ripe to begin actual experiments in this field".

The following is an extract from the paper, omitting only some of the formal analyses:

> "The further development of mineral mining by the open-cut method is planned by the directives of the 20th Congress of the Communist Party of the Soviet Union. To expose strata of coal or mineral deposits

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1. "On the Use of Nuclear Explosions for Industrial Purposes" by G. I. Pokrovskiy, Gornyy Zhurn 1, Moscow, No. 5, 1956, Pp. 29-32.

which are to be exploited, in the majority of cases, it is necessary to remove huge masses of gangue. This problem can be solved by various methods. Among them, massive explosions attract attention. Recently, the Soyuzvzryvprom Trust (Union Explosives Industry) successfully conducted a series of mass explosions, the weight of separate charges being as high as 1,640 tons. There is reason to believe that in the future it will be expedient to use even larger charges for blasting operations. Preliminary plans exist for ejection blasts using concentrated charges weighing from several thousand up to 250,000 tons.

"The realization of such plans may encounter sizeable difficulties, not so much from the economic as from the engineering aspect. If for a ton of explosive (with some minimization) a chamber not less than one cubic meter in volume is required, then for a charge with a weight of 250,000 tons, a cubic chamber having a width and height of 65 meters is required. It is obvious that a chamber with such a span, as a rule, cannot be formed underground since no reinforcement will withstand the tremendous mineral pressure.

"The same conclusion might be made for smaller charges, for example, for a charge with a weight of 10,000 tons, a cubic chamber with a span of 22 meters is necessary; this also, is unacceptable in many cases. It is certain that the use of huge quantities of explosives for the performance of operations at different points may cause considerable difficulties in production and transport. Thus, development of the blasting business requires essential engineering innovations.

"One possible course is the use of nuclear explosives for blasting. As is known the following may be used for nuclear explosions: Plutonium, produced in nuclear reactors; uranium-235, extracted from natural uranium; and uranium-233, obtained from thorium, and also heavy hydrogen (deuterium), extracted from water; extra-heavy hydrogen (tritium), produced in reactors; and finally, lithium.

"Plutonium, uranium-235, and uranium-233 can be exploded on obtaining the so-called critical mass by rapidly bringing together its parts, initially

located some distance from each other, by the explosion of an ordinary explosive.

"The dimensions of atomic charges with all auxiliary equipment are not large. Thus, the caliber of the atomic shell of the American cannon (Bol'shaya Sovetskaya Entsiklopediya (Large Soviet Encyclopedia), Vol. 35, 1955, P. 335) is 280 mm and weighs about 360 kg. Considering the substantial case necessary for the shell, it can be assumed that the contained atomic charge with all accessories, the trinitrotoluene equivalent of which is about 14,000 tons, has a diameter of about 250 mm and weighs no more than 100-200 kg.

"Such a charge may be easily lowered into an ordinary drill hole with a diameter of 270-300 mm. The use of a TNT equivalent several times greater for an increase in the explosive energy presents no technical difficulties. The problem of placing a charge corresponding to tens-of-thousands of tons of TNT at the necessary depth is as easily solved.

"However, a sharp reduction of the TNT equivalent in an atomic explosion in comparison with the mentioned magnitude has not yet been attained because a definite critical mass of uranium or plutonium is necessary for an atomic explosion. Therefore, for the present, one cannot talk of using atomic material for ordinary explosive operations. On the other hand, further increase in the scale of explosions is possible through atomic engineering.

"There is an especially good outlook for strengthening a plutonium or uranium charge by adding to it deuterium and tritium along with lithium. The temperatures to tens-of-millions of degrees arising in the explosion of a plutonium or uranium charge are sufficient to cause the fusion explosion of heavy hydrogen and lithium.

"Thus not only uranium, which is already used for production of electrical energy, but also thermonuclear fuels, the supply of which is practically limitless in nature, can be used for productive purposes. However, up to now, nuclear fuels could be used only for military purposes -- to make the so-called hydrogen bombs.

"Nuclear (atomic and thermonuclear) explosions can be used not only to construct huge pits for mining minerals, but also for transportation and hydrotechnical construction.

"The cost of atomic and thermonuclear explosives is difficult to determine with sufficient objectivity at present. Experience in operating the first atomic electric power station in the world, the station of the Academy of Sciences USSR, shows that the cost of electrical energy obtained in this way can, in the near future, be comparable with the cost of energy yielded by thermal electric power stations. The cost of atomic and thermonuclear explosives is higher than the cost of atomic fuel (for electric power stations). However, it might be assumed that even now the over-all cost of work involving atomic explosions with TNT equivalent greater than 10,000 tons will not be greater than the cost of work with ordinary explosives on such a scale. Undoubtedly, considerable savings will be realized in the time and expense connected with preliminary work.

"Let us consider the approximate expense of a nuclear explosion where a charge with TNT equivalent 15,000 tons is placed at a depth of 150 meters. Using ordinary explosives, it is necessary to sink a shaft 150 meters deep with a chamber about 15,000 cubic meters in volume. In an unfamiliar area, a mining operation of such a nature would require 8-12 months. The shaft would cost about 300,000 rubles, and the chamber would cost about 7.5 million rubles; thus, the over-all cost of the preparatory work would approach 8 million rubles.

"If nuclear explosives are used, it will be necessary to drill only a bore 150 meters deep. Not more than one or two months will be required for this (even including preparatory work). The cost of the bore will not be more than 50,000 rubles. This total is negligibly small in comparison with the expense in preparing for the usual explosion.

"The cost of 15,000 tons of ordinary explosives is approximately 30 million rubles. Thus the cost of an ordinary explosion on the

scale in question would approach 40 million rubles. Although the coefficient of usable potential of nuclear explosion is assumed to be only several percent, nevertheless, one can say that under current conditions the cost of nuclear explosives (several kilograms of plutonium or uranium or an even small amount of thermonuclear fuel) would be less than an equivalent amount of ordinary explosives.

"A somewhat more complex question in the application of nuclear explosions to mining and construction is contamination of the blast area and surrounding regions by the radioactive decay products of the nuclear charge and radioactive substances which form in the surrounding area owing to the action of neutrons emitted in a nuclear explosion. This problem also includes the safety of the personnel performing the blasting operation and the time required before work can be resumed in the blast area.

"To clarify the basic phenomena causing radioactive contamination in an atomic explosion, we should first of all note that immediately after the explosion, before any substantial displacement of the rock has taken place, radioactive contamination is confined to the zone where melting and evaporation of the rock have taken place. (Note: A calculation was made that indicated the radius of rock melted by 15000 tons would be about 17 feet.) It follows from this (the calculation) that, at the initial stages in the development of the underground explosion, there can be no break-through of gases bearing radioactive substances.

"Analysis of rock movement in succeeding stages of a blast shows that the small mass initially adjacent to the charge is intermixed with almost all the rock which is thrown from the crater and which partially falls back into it. This process of rock redistribution leads to the situation in which radioactive contamination is spread throughout the whole crater and far from its boundaries, through almost all of the zone covered with the scattered rock.

"This situation also means that part of the radioactive material is distributed at depths greater than one meter. This material is not

dangerous, because the rock lying over it absorbs the emitted radiation. Only the upper layer of ground, for a thickness of about 0.5 meters, is dangerous to people on the surface.

"As a result, 80-90 percent of the radioactive material does not present any danger.

"In an underground nuclear explosion with a wind present, we should expect the development of a fairly wide zone of radioactive contamination, drawn out in the direction of the wind. This question requires further first-hand study with actual explosions.

"With the data now available, however, we can say that radioactive contamination in a nuclear explosion should not be considered an insurmountable obstacle to the use of such explosions in mining and construction. On the basis of the many advantages of nuclear explosions, we conclude that the time is ripe to begin actual experiments in this field."

There are several significant points made in the paper. These are:

1. Placement of 15,000 tons of explosives is assumed to be at a depth of 150 meters (500 feet). This corresponds to a scaled depth of 200 feet, which is about what U.S. experience since Neptune (October 1958) would indicate to be the proper depth. For Chariot, the present plan contemplates using a depth of placement of 400 feet for a 20-kiloton charge.

2. The cost of a 15,000-ton nuclear explosive, which would contain several kilograms of plutonium or uranium, would be less than conventional high explosive (30 million rubles). For the example used, the placement cost for conventional high explosive was estimated to be 15 times greater than for a nuclear explosive. Thus at for 10 kilotons and above based on this analysis, from an economic standpoint, it becomes advantageous to use nuclear explosives.

3. The bulk (80-90%) of the radioactivity would be trapped underground and would not represent any danger. (Based on Nevada experience under the conditions of this explosion, it would be expected that more like 99% of the gross activity would be retained.)

4. With data presently available, radioactivity should not be considered an obstacle.

5. Finally it was stated that preliminary plans exist for high-explosive experiments using concentrated charges from several thousands of tons up to 250,000 tons, and it was considered that the time was ripe for nuclear experiments.

Let us now review, from what has been published, what the accomplishments of the Soviet Union in this field in recent years have been. The experience has been extensive and the program appears to be an implementation of the directive of the 20th Congress of the Soviet Union. The explosions with one exception have been conducted in support of large excavation projects, but they comprise precisely the kind of experiments that would be done in support of an aggressive Plowshare program.

The greatest blastingever undertaken for peaceful purposes were executed under the direction of Soviet specialists in China during 1956. The charges were fired to strip overburden from underlying ore deposits to permit pit mining. The details of placement and firing of the experiments have not been published, as far as has been determined, nor have the detailed results been reported. However, the assumption is that they were concentrated charges which implies that the entire charge was detonated at one time. The magnitudes and dates of firing are as follows:

July 19, 1956.....1,640 tons
Nov. 16, 1956.....4,780 tons
Dec. 31, 1956.....9,200 tons

The results of these explosives were recognized as major contributions to the development of theory and pointed the way to next steps. It was concluded that a highly instrumented experiment be designed or executed to provide more details on cratering phenomena. Accordingly, a 1000-ton experiment, using high explosive, was conducted at Tashkent on December 19, 1957.

Some description of the experiment and results were published[2,3] though many important details are not available. In the latest publication (Dec 1959)

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[2] "Diffusion Blasting", G.E. Pokrovskiy, The Military Aeronautical Engineering Academy (Moscow) August 1957.

[3] "The Difference in the Periods of Seismic Waves Excited During Underground Explosions and Earthquakes", S.D. Kogan, I.P. Pasechneck and D.D. Sultanov Reports Proceedings of the Academy of Sciences of the USSR, Vol. 129, No. 6, Pp. 1283 - 1286, December 21, 1959.

we have the following description:

> "On December 19, 1957 at 0900 GMT, 30 kilometers southeast from the station of Arys on the Tashkent railroad (42° 12' 15" N; 16° 32' 59" E) there was carried out for scientific purposes an explosion of 1,000 tons of chemical explosives. The charge was placed in a chamber of a shaft which had been dug in a clay soil at a depth of 40 meters below the surface. During the explosion a crater was formed with a depth of 50 meters and a radius of 100 meters."

That this was a part of the Soviet Program for industrial uses was clearly indicated by Moscow press release (June 13, 1958) which, in describing the results, stated, "Test results proved that the force of large-scale blasting can be utilized for the construction of canals, ditches, trenches and reservoirs, in accordance with plans formulated in advance." Further on it was stated that "*** persons in charge of blastings will soon be able to lift an entire mountain into the sky, converting mountains into plains."

With the calibration available from the 1,000-ton instrumented experiment, the Chinese single charge results, and a series of smaller high-explosive cratering experiments, the Soviet scientists derived a scaling law which is in agreement with the more limited U.S. experience.

The basic laws for concentrated charges had now been developed. What was needed next were experiments in rows of point charges to develop the laws of spacing of such charges and depths of placement to dig ditches and canals. To this end an experiment of 3,100 tons was carried out on March 25, 1958. The following quote is from a recent Russian report[3]: "On March 25, 1958 at 0900 GMT in the district of Pokrovsk Ural' Skii (60.2° N. 59.9° E) 3100 tons of chemical explosives were detonated for the purpose of excavating a canal having a length of 1100 meters. The charge was placed in 31 holes varying in depth from 10 meters to 15 meters. The soil was rocky and saturated with water."

Earlier in Pravda (March 26, 1958) a report of the same experiment appeared as follows:

> "Svenouralsk, Sverdlovsk Province - The largest rock clearing

explosion in the history of the Soviet Union was set off March 25 near the workers' settlement of Pokrovsk Uralsky. The explosive charge weighed 3,100 tons.... In a few seconds the explosion accomplished a vast amount of work. It carved an outlet canal for the Kolonga River 1,150 meters long, as wide as 100 meters at the top and 25 meters deep. About 750,000 cubic meters of earth and rock were blasted out.

"Why was this explosion set off? The Kolonga River flows over rich deposits of iron ore. Ore-filled trains constantly ply between the river and Serov Metallurgical Combine. However, there is a grave flooding problem in the mines, making the ore difficult to extract. The May Day Mine, which was built in 1957 has yet to be put into operation because of the great flow of water.

"So, the Pokrovsk miners, together with the Urals Explosives Trust, decided to set off three powerful blasts in order to open up a 2-kilometer outlet canal for the Kolonga River. The first and second explosions, which were set off in April and December, 1957, made a cut 798 meters long for the Canal.

"The third explosion, the March 25 one, was prepared with particular care. Engineer A. Smirnov, who drew up the project, and the others in charge of the work planned and carried out the blast on a high technical level. The explosion was detonated only one and one-half to two kilometers from the workers' settlement, but the settlement's buildings suffered no damage at all. The explosion was so cleanly carried out that no finishing work is needed on the canal. In a few months the Kolonga's waters will be closed off by a dam and will flow through the craggy hills and into Lake Troitskaye."

This experiment was the end of a series of three at the same site. The other two were fired in April and December of 1957, but no results are available. From the statement that the "*** explosion was so cleanly carried out that no finishing work is needed on the canal", it can be concluded that the understanding of the proper placement of charges from previous experiments can be considered correct.

The U. S. Program includes studies of spaced charges for canal construction but is still relatively small. Rows of charges up to 1000 pounds only have been employed to date.

The experiments underway are designed to give a guide to the proper placement for the 20-kiloton nuclear explosions of the proposed Chariot experiment where four such charges will be placed in a line.

If it is assumed that the Russian explosions were equally spaced and of the same magnitude, each charge would have been 100 tons. Having available 31 shot points, the Soviet scientists could easily have varied spacings and depths (which they did) to develop the maximum amount of data. Unfortunately, the details of the experiment and the results have not been published. Apparently the 100-ton charges were spaced at intervals of about 35 meters (117 feet) and at depths of 10-15 meters (33-50 feet). From U. S. data, under these conditions, the 100-ton shots placed at depths of 15 meters (50 feet) would use a spacing of 24 meters (80 feet). This would produce in a desert alluvium a channel with the dimensions 48 meters (160 feet) wide at the top, 13 meters (45 feet) deep, and 720 meters (2400 feet) long. These dimensions are considerably smaller than those reported by the Russians but could be due to difference in soils at the two sites.

What cratering results to expect in a rocky medium, particularly granite or basalt, is not known. Presently underway at the Nevada Test Site as part of the Plowshare Program are experiments with 1000-pound charges in basalt to establish the first rough curves of crater dimensions as a function of depth of burst.

The Soviet Union now has experience in chemical charges to produce craters or canals in energy release from a few pounds up to a few thousand tons. From this the scaling laws with HE are developed from very low energies to energy releases characteristic of the nuclear range. Range of energies covered by the experience is therefore at least a millionfold, and the energy releases are clearly in the nuclear range. All that is now required is nuclear experience to obtain the cratering efficiency with respect to chemical explosives and the data on the effectiveness of containment of radioactivity. The proposed U. S. nuclear experiment in excavation, Project Chariot, hopefully will provide much of the necessary data.

Whether the Russians have nuclear experience to compare with the HE program is not known. The only reference made officially to such experience was the 1949 Vishinsky statement and no technical data have ever been published on those explosions or any subsequent ones that might have been conducted as part of the program.

When asked at a press conference during the second Atoms for Peace Conference in Geneva in 1958 whether the Russians had a program similar to the Plowshare, Professor V. S. Emelyanov had this to say: "I, personally, think nuclear explosives do not reach practical ends, only political ones, and I am in favor of eliminating them..... I would not spend a kopek for such tests.... We have no project or plans to use nuclear explosives in this way." When reminded of the 1949 Vishinsky statement, he replied he knew nothing of it because he had only entered politics in 1955.

That, at least, the chemical explosive program is still being continued vigorously is attested to by this Moscow press release of March 5, 1960.

EXPLOSIONS AND THE NATIONAL ECONOMY

"(Text) Novosibirsk -- A council for the use of explosions in the national economy has been set up under the Siberian Branch of the USSR Academy of Sciences, today's Vecherny Novosibirsk reports.

"The Siberian scientists propose to strip a 45-meter thick coal seam at the recently discovered south Yakutian coking coal deposit with the help of two big explosions -- 40,000 tons of ammonal each. The seam lies at depths of 40 - 60 meters under a layer of soft sand. This tremendous explosion can also yield valuable information on the geological structure of this part of Siberia. The cost of explosion stripping will be several times smaller than the usual methods.

"The scientists also propose to build a dam with the help of explosives in the Malaya Alma-Atinka River Gorge in Kazakhstan, a few kilometers above the Medeo Mountain Skating Rink. This dam will shield the Republican capital of Alma Ata from the destructive mud and rock streams which rush down from the mountains approximately once every 50 years.

The dam is to be almost 100 meters high. It will be built by blasting the gorge sides, which will require the simultaneous detonation of approximately 5000 tons of explosives. To raise the dam to the projected mark, it is necessary to make two smaller explosions.

Explosion methods, the article continues, open up great prospects for the deepening of river beds. The volume of these works on the Siberian rivers alone will amount to many hundred of thousands of cubic meters in the current seven-year-plan period, 1959 - 1965.

The scientists have advanced a new theory of destroying the ice on waterways with the help of explosions. They have also developed a unit for drilling shot holes in hard rock with a large number of continuous blasts. A special fluid is used as an explosive."

Because the results of these experiments and projects are of fundamental interest to Plowshare, the Soviet Union could make a major contribution to peaceful applications and science if it would provide the United States and the world with all technical data from these important explosions. Specifically, information is needed on the details of placement and distribution of charge, the kind of explosives used, the manner of firing, the nature of the medium, engineering surveys of the craters, the close-in ground-motion measurements and seismic observations.

G. W. JOHNSON
LAWRENCE RADIATION LABORATORY
MARCH 28, 1960

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(Whereupon, at 5:10 p.m., Tuesday, March 22, 1960, the subcommittee recessed, to reconvene at 10 a.m., Wednesday, March 23, 1960.)

FRONTIERS IN ATOMIC ENERGY RESEARCH

WEDNESDAY, MARCH 23, 1960

CONGRESS OF THE UNITED STATES,
SUBCOMMITTEE ON RESEARCH AND DEVELOPMENT,
JOINT COMMITTEE ON ATOMIC ENERGY,
Washington, D.C.

The subcommittee met at 10 a.m., pursuant to recess, in room P–63, the Capitol, Hon. Melvin Price (chairman of the subcommittee) presiding.

Present: Representatives Price, Holifield, Van Zandt, and Bates; Senator Aiken.

Also present: James T. Ramey, executive director; George F. Murphy, Jr., professional staff member, and Edward J. Bauser, technical adviser, Joint Committee on Atomic Energy.

Representative PRICE. The committee will be in order.

This morning our subject is the Sherwood program which is the research program aimed at the development of controlled thermonuclear reactions for energy generation.

The potentials of this development for the welfare of mankind are enormous. Success will give mankind a limitless source of energy. The research is aimed at tapping the same reaction which occurs in the sun and stars to generate energy. A whole new field of physics called plasma physics has been developed only in the last decade on this frontier of science.

This morning we have with us leading scientists from our national laboratories who will tell us what the status and prospects are of this fascinating field of science. We also have with us Dr. Paul McDaniel and Dr. Arthur Ruark of the AEC who are responsible for the Sherwood program in the Commission.

Our first witness will be Dr. McDaniel.

STATEMENT OF PAUL McDANIEL,[1] DIVISION OF RESEARCH, ATOMIC ENERGY COMMISSION

Dr. MCDANIEL. Thank you very much, sir. I have a short prepared statement, and I propose to read that, and then answer any questions you have.

[1] Born: Jan. 1, 1916, Robards, Ky.
Married: June 4, 1937.
Children: None.
Citizenship: United States.
Education: Bachelor of science, Western Kentucky State College, 1936; M.S. in physics, Indiana University, 1938; Ph. D. in physics, Indiana University, 1941.
Positions: Assistant physics, Indiana 1937–41; physics instructor, Alabama Polytechnic Institute, 1941–42; professor physics, 1946, 1953; major, U.S. Army, 1942–45; Chief, Technical Branch, Research Division, Manhattan Engineer District, 1945–47; Chief, Information and Materials Branch, Division of Research, U.S. Atomic Energy Commission, 1947–48; Executive Officer, 1948–50; Deputy Director, 1950–59; Acting Director, 1959–60; Director, 1960.
Professional and honorary societies: American Physical Society. Association Physics Teachers, American Association for the Advancemeent of Science, American Nuclear Society, Sigma Xi.
Field of study: Nuclear physics.

Representative PRICE. You go right ahead, Dr. McDaniel.

Dr. MCDANIEL. In February 1958, we had the pleasure of assisting in the presentation of the physical research program of the Atomic Energy Commission to the Subcommittee on Research and Development of the Joint Committee on Atomic Energy. At that time, about 50 leading scientists from our national laboratories, research institutions, and universities, large and small, told you what they had done, what they were doing, and what they were hoping to do. Almost all of the scientists who participated in these hearings have told me of their appreciation for the opportunity this committee gave them to explain their work.

On that occasion some of our scientists working on controlled thermonuclear research presented their results. Only the research on pinch discharges had then been declassified. But such work was only one of several approaches for our scientists. They were then carrying on five types of research to find out whether the energy locked up in the isotopes of hydrogen could be released in a controlled manner to provide abundant electric power. Later the programs in this country and in the United Kingdom were completely declassified, just in time for the Second Geneva Conference on Peaceful Uses of Atomic Energy in September 1958. We took several of our full-scale thermonuclear devices to Geneva. Scores of American scientists and technicians went to Geneva to operate this equipment, to explain their results, and to make friends with scientists of other nations. Several members of the Joint Committee on Atomic Energy were present at the Conference. They saw these exhibits and they heard our scientists describe their results. But today for the first time we have the opportunity to bring the thermonuclear story up to date before this committee. I am sure that the committee will find the testimony of Dr. Ruark and his colleagues to be most interesting and I shall leave it to them to describe the program in detail. However, I would like to comment briefly on the nature and extent of the work being done both here and abroad.

At Geneva, it became clear to me that most of the ideas on fusion power of American scientists and their European colleagues had also been conceived by scientists from the Soviet Union. Conversely, the Soviet Union did not seem to be working on any new ideas which had not been known and considered here. This illustrates once again the basic similarity of scientific endeavor as practiced by first rate scientists anywhere. I wish to devote the remainder of my time to a broad survey of the world effort on controlled fusion research.

Large experimental and theoretical groups are working in France, West Germany, the United Kingdom, the Soviet Union, and the United States. Smaller organized efforts are going on in Australia, Denmark, Italy, Japan, Netherlands, and Sweden. In the United States, we have about 350 scientists working in this field. While we have no reliable manpower figure for the Soviet effort, it is clear to me from personal contacts with the Russian fusion scientists and from consideration of their publications that the scientific effort in the Soviet Union is roughly comparable with our own or with the combined efforts of the United Kingdom and Western Europe. In Western Europe, a study group of scientists from 13 nations supporting the CERN laboratory in Geneva, Switzerland, have twice considered the

question of a "central laboratory" in Europe, devoted to this subject. They concluded in each case that the formation of such a laboratory was not warranted. The European effort is thus confined to the efforts of the individual countries.

The British have a large pinch machine called ZETA at Harwell. When it became apparent that this machine would not reach their original expectations, they altered their plans and have been designing a different type of large pinch machine called ICSE, which stands for intermediate current stability experiment. The British are planning for a new laboratory at Culham, near Oxford, to accommodate all the controlled thermonuclear research of the United Kingdom.

Experimental efforts in France and Germany began with a multiplicity of pinch devices. These devices make it possible to obtain hot plasma with minimum expense and trouble. As in the case of the United Kingdom, there is an increasing interest in other heating methods. In France, about 60 scientists are working in the program and they are becoming interested in devices related to our DCX–1 at Oak Ridge. In West Germany, about 110 scientists are working in the controlled thermonuclear field. Some of these are working at Munich on a device somewhat like our stellerators.

The Soviet Union has supported a multiple approach. The device of greatest current interest is perhaps the large magnetic mirror known as OGRA. The Russians have advised us informally that they have completed a round of preliminary experiments on this machine and that they plan to increase the injection energy and current of the entering ions.

It should be recorded that the United Kingdom, the larger nations of Western Europe, and the Soviet Union are placing relatively heavy emphasis on controlled fusion. Premier Khrushchev, in discussing the chief goals for Soviet scientific effort during the 21st Congress of the Communist Party, meeting in the winter of 1958 and 1959, mentioned this line of research more than once. President Nesmeyanov of the Soviet Academy of Sciences said in discussing nuclear physics:

> A great deal of attention will be devoted to further research into the so-called high energy nuclear collisions, as a result of which new elementary particles make their appearance, as well as the problem of controlled thermonuclear reactions.

I believe that in the field of controlled thermonuclear research, the United States continues in a position of leadership. The witnesses who will follow me will give you a more adequate idea as to where we stand today. These people from our laboratories are inspired men. They are led on by the great vision of abundant power from the unlimited store of nuclear energy in the oceans of the world. It must not be supposed, however, that our scientists or those of any other nation are likely to achieve startling results quickly in this field of research, through some stroke of good fortune. Rather, it is a matter of gradually increasing capabilities. Month by month and year by year, our laboratories and those of the other nations will forge steadily ahead.

That completes my prepared statement.

Representative PRICE. Thank you very much, Dr. McDaniel. Doctor, do you feel there is good exchange of information between the Sherwood people and their counterparts in other countries?

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Dr. McDaniel. I think, sir, this field is perhaps the one field in which there is almost complete exchange between all countries of the world, including the Soviet Union.

Representative Price. You said in Europe that the people who were working on fusion at one time considered a central laboratory, and then there was a little opposition to this, and it did not materialize. What is your opinion about the United States?

Dr. McDaniel. In any subject like this, sir, there are two sides. There are some advantages to a central laboratory when one attacks a major problem. However, in this country we have four major groups concerned with somewhat different approaches, and it is my opinion that it would be wrong to attempt to consolidate the activities of these people into one central laboratory. There would be years of delay. People are involved in such moves. The spirit of competition which we have among our laboratories and all of these things make it, I believe, not proper to consolidate them into one place.

Representative Price. Thank you very much, Doctor.

The next witness will be Dr. Ruark. Would you proceed with your statement, please?

STATEMENT OF ARTHUR E. RUARK,[1] DIVISION OF RESEARCH, ATOMIC ENERGY COMMISSION

Dr. Ruark. Mr. Chairman and members of the subcommittee, I, too, have a prepared statement to read to economize your time.

Dr. McDaniel has emphasized the human importance of Project Sherwood. It also has a great intellectual challenge. The work is just plain tough, and scientists like to bite into problems which strain their capabilities. It is my pleasant task to make some remarks on our general status, and our expectations.

So far as can be seen from our present knowledge of natural forces, the only way to make a fully controlled thermonuclear reaction is to confine a very hot and very tenuous gas in a vacuum vessel with the aid of a magnetic field. A sufficiently hot gas, or plasma, is made up of electrons and bare nuclei. As they carry out their heat motion, the magnetic force curls their paths into complicated spirals. In this way they are retained in the vacuum vessel instead of flying to the walls. Then the nuclei can collide, time and time again, until some of them manage to coalesce and produce other nuclei, which fly apart with

[1] Born: Nov. 9, 1899, Washington, D.C.
Married: Sarah Grace Hazen; 4 children.
Education: B.A., 1921; M.A. in physics, 1923, Ph. D. in physics, 1924, the Johns Hopkins University.
Experience: Atomic Structure Section, Bureau of Standards, 1922–26; assistant professor of physics, Yale University, 1926–27; physicist, Gulf Oil Co., Mellon Institute, 1927–29; chief of physics division, Gulf Research Laboratory, 1930; professor of physics, University of Pittsburgh, 1930–34; head of physics department and Kenan professor of physics, University of North Carolina, 1934–44; consultant, Naval Research Laboratory, 1944–46; head of research laboratory division, Applied Physics Laboratory of the Johns Hopkins University, 1946–47; assistant director, Institute for Cooperative Research, the Johns Hopkins University, 1947–49; research contract director, Institute for Cooperative Research, the Johns Hopkins University, 1949–52; Temerson professor of physics, University of Alabama, 1953–56; Chief, Controlled Thermonuclear Branch, Division of Research, U.S. Atomic Energy Commission, 1957–.
Fields of specialization: Atomic physics; physics of fundamental particles, wave mechanics; interaction of electrons with matter; nuclear physics.

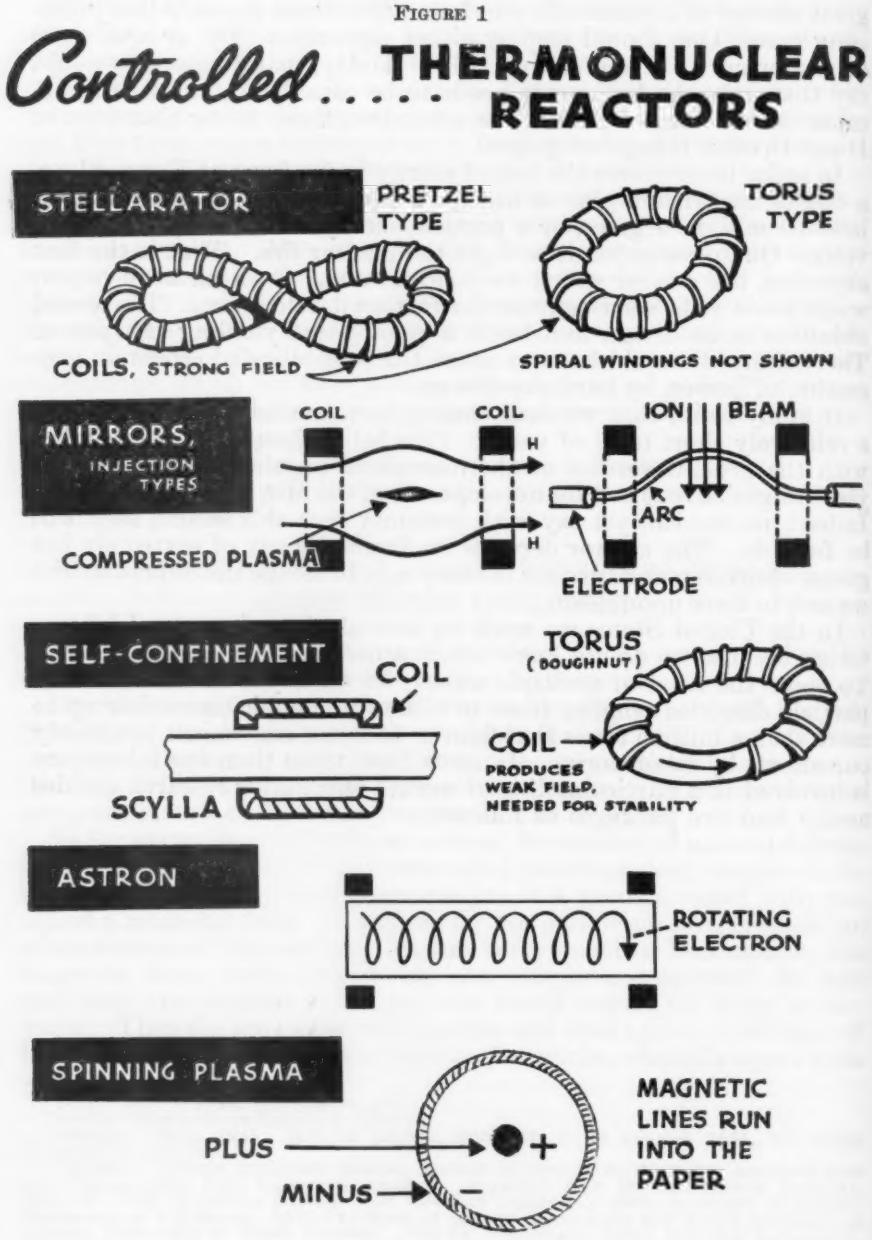
great energy of motion. It was found that there are only two promising cases: One should employ either deuterium (D), or a mixture of deuterium (D) and tritium (T). Today, many scientists would say that only the latter case needs to be considered, principally because the reaction of D and T is a hundred times faster than that of D and D, other things being equal.

In order to overcome the loss of energy in the form of X-rays alone, a deuterium-tritium plasma has to be heated to a temperature of at least 50 million degrees, or a particle energy of about 5,000 electron volts. Our present job is to light the nuclear fire. That is the first objective, the one on which we concentrated. Such an achievement would be only the starting point for further development. The second objective is to design and build a pilot plant yielding net power. Thereafter, we would have to assess the possibility of economic generation of power, by hard experience.

It is my belief that we shall obtain thermonuclear temperatures in a relatively short term of years. This belief should not be confused with the general verdict of thermonuclear physicists that 10 to 20 years might be required for development of the first fusion powerplant. Indeed, no one can yet say with certainty that this second step will be feasible. The answer depends on further study of extremely hot gases, squirming and turning in every way to escape the imprisonment we seek to force upon them.

In the United States we work on several plans, in several laboratories, because we do not know which attack may be most successful. To show the need of multiple attack, let me say that we deal with particle densities ranging from 10 billion in a cubic centimeter up to more than a million times that figure. It is not convenient to classify our efforts by laboratories. In some cases more than one laboratory is involved in a particular line of work. Our major research divides neatly into five packages as follows:

FIGURE 1


Controlled..... THERMONUCLEAR REACTORS
STELLARATOR
PRETZEL TYPE
TORUS TYPE
COILS, STRONG FIELD
SPIRAL WINDINGS NOT SHOWN
MIRRORS, INJECTION TYPES
COIL
COIL
ION
BEAM
H
H
ARC
COMPRESSED PLASMA
ELECTRODE
SELF-CONFINEMENT
COIL
TORUS (DOUGHNUT)
COIL
PRODUCES WEAK FIELD, NEEDED FOR STABILITY
SCYLLA
ASTRON
ROTATING ELECTRON
SPINNING PLASMA
PLUS
MINUS
+
−
MAGNETIC LINES RUN INTO THE PAPER

1. Stellarators: These are endless tubes like hollow doughnuts, used at Princeton.

2. Injection into magnetic mirrors: Particles of high energy are injected into these straight magnetic bottles, at Livermore and Oak Ridge.

3. Self-confinement methods: Strong currents are passed through the gas, which is confined by the magnetic field of these currents. These methods are practiced at Los Alamos, Berkeley, Livermore, and the Naval Research Laboratory.

4. Astron: Currents of very fast electrons are employed to confine and heat the plasma, at Livermore.

5. Spinning plasmas: In this case, both electric and magnetic fields are employed. Small efforts go on at Los Alamos and Berkeley.

Let me mention briefly the status of just a few devices.

At Princeton we are building the model C stellarator which should come into action next winter.

At both Los Alamos and the Naval Research Laboratory there are shock-mirror devices using dense plasmas. The one at Los Alamos is called Scylla. The one at Washington has no name. Perhaps it might be called Narcissus, for that Greek fellow who was so anxious to look at himself in mirrors. These devices now operate at particle energies only about three times lower than the ignition energy. At both places, new condenser banks now being built should enable these machines to produce much higher particle energies.

At LRL there is a rather old mirror machine called Table Top. It has produced a plasma with an electron energy of 25,000 electron volts. Due to its design, the ions were at much lower energy—not a striking energy at all—but the plasma was stable for 10 milliseconds. This stability is the point of real interest.

At ORNL we have DCX–1, with provisions for injecting ions of 600 kilovolts, far above the value necessary for effective thermonuclear reaction. A better machine, DCX–2, has been designed. Oak Ridge now has a power supply for ion injection with a rating of 1 ampere at 600 kilovolts. It is the most powerful supply of its kind in the world.

The Astron, too complex for detailed description here, uses an electron beam of 50 amperes or more at 600 kilovolts.

Our scientists have the equipment and the skills to go on up the line toward the ignition energy. The totality of their achievements is impressive. I state without fear of contradiction that in this field we have an outstanding position. We intend to hold onto it.

Representative PRICE. Dr. Ruark, I would like to get your idea of what amount of effort the Russians may be putting behind a similar program as we have in Sherwood.

Dr. RUARK. They certainly have very good people and, like our own attack, their attack is a multiple one. They do not have just everything we have, and when we come down to the level of the smaller experiments, of course there can be no exact similarity. I mean, referring to the larger machines, that they do not, for example, have anything, to the best of our knowledge, like the Astron—or anything like the stellarator. But from what we know of their effort, we believe that in manpower and total bodies of equipment and power

supplies, it is indeed quite comparable. I would not want to say closely equal, but quite comparable, sir.

Representative PRICE. In your position in the Commission, are you in a sort of a coordinating position between the various phases of this program?

Dr. RUARK. In the United States, yes, sir. We interact also with the other nations.

Representative PRICE. What is your observation of the physical properties that the various programs have to work in? Do we have adequate facilities for the people who are working on this program?

Dr. RUARK. I would say yes, sir, at the present time, including the items that are still in process of completion. At this time, at all four of our laboratories, adequate space has been provided. As far as power supplies are concerned, new and stronger ones—like the ones at Princeton, Oak Ridge, and Los Alamos—are either complete or approaching completion.

Representative PRICE. I know it is a little early to ask, even though we have been working for years on this, as to when we might attain fusion power. Do you have a guess on that?

Dr. RUARK. In this brief presentation, I quoted a range of figures that I am likely to hear as I go around among the scientists. I said the general consensus in the group seems to run from 10 to 20 years. Others have sometimes estimated much higher values. I think this is somewhat a function of the degree of one's responsibility and conservatism.

Representative PRICE. As you circulate among the different groups working in the program, do you run across any that have any misapprehensions that they will run into any basic physical law that would prevent us from ever attaining fusion power?

Dr. RUARK. In this field it is a part of our business to look for these troubles. From almost the very beginning most of us had lists of troubles, big ones, that we would like to study, to see if any of them would knock the whole thing into a cocked hat. One by one these difficulties fell away until I think most of our people would agree that the question of instabilities of the plasma, of its quietude or turbulence, is really the determining factor. I could put it another way. If you assume a quiet plasma, all the dreamboat designs, all the calculations you can make as to the properties of possible future reactors, turn out in general on the favorable side. The machines appear feasible with quiet plasmas. But will the plasmas be quiet? It is like asking what the boys will do at a high school when recess time begins. Usually they become rather turbulent during a recess period.

Representative PRICE. Mr. Van Zandt.

Representative VAN ZANDT. Doctor, at Geneva a few years ago, the Russians came forward with some kind of a thermonuclear device. What actually developed since then as far as the Russian device is concerned?

Dr. RUARK. Mr. Van Zandt, I think you must be referring to the OGRA machine which we learned about only on the eve of the conference. That machine is still in operation with various improvements. I think perhaps you were out of the room when I was reading a sentence or two about this which will appear for you in the record, but I will be glad to repeat. The machine has gone through a round

of preliminary, very interesting experiments, and some of them are very puzzling. It is a long mirror machine, the largest in the world. Because it is large, it is hard to work with. At this time they realize that in order to reach results more favorable toward the creation of a thermonuclear plasma, they must increase the current, and they must increase the voltage on the injected ions. When they were here with Emelyanov in November, that is exactly what they told us they were going to do. That is what we expect to do in our mirror programs.

Representative VAN ZANDT. In other words, we have had an exchange of information regarding it?

Dr. RUARK. Yes, sir, a good exchange of information in November.

Mr. RAMEY. They were particularly interested in Dr. Tuck's machine, were they not, the injector that Los Alamos has developed?

Dr. RUARK. They were really an eager and enthusiastic group. If you refer to his new trapping machine, yes, that was something fresh and new for them to see. But they surely showed a general interest in all of our laboratories.

Representative PRICE. Are there any further questions?

Representative VAN ZANDT. Did we show the Russians the model C we are building at Princeton?

Dr. RUARK. Yes, sir. They have seen it very well, indeed.

Representative VAN ZANDT. How about Dr. Tuck's development at Los Alamos? Did they get a complete picture there?

Dr. RUARK. Yes. I was there with them and I can certainly say that at all our laboratories, so far as the time permitted, everything was shown them.

Representative VAN ZANDT. Are you convinced, Doctor, we saw everything to be seen in Russia when our delegations visited that country?

Dr. RUARK. I would like for Dr. Gottlieb to comment on that when he comes up for his talk or at any time at your pleasure. He was on our first group that went there and can speak also for his colleague, Dr. Spitzer. They went only to two cities having thermonuclear laboratories. The Russians, like ourselves, have four main places, and our people—just for reasons of timing on these visits—have so far visited only in Moscow and Leningrad.

Representative VAN ZANDT. Assuming you have all the information available that was brought back to this country by the group who visited Russia, are you of the opinion the exchange is an equal one? In other words, we have practically given all the information we have in this field to them, and have we received in return all the information they have in this field?

Dr. RUARK. To the first part I can say that we are entirely unclassified, as you know, that our publication facilities are excellent and prompt, and that there is a flood of papers, particularly in the journal, "Physics of Fluids." Sometimes the whole journal issue has been devoted to thermonuclear papers. There is every opportunity for them to learn everything that we do.

As to the second part, I cannot quite say, just because of the usual difficulties of communication across the ocean, and the fact that their writings are in the Russian language and may remain somewhat

unavailable for the reason that we do not have enough people reading Russian. But we feel well satisfied with what they publish.

Representative VAN ZANDT. A year or more ago, Doctor, we were confronted with the problem of translation. We had, I understand, several years of accumulation of documents that awaited translation. Do you have any information as to whether or not we are overcoming this problem?

Dr. RUARK. I would say there is a continuing effort. The effort has been mainly along the lines of arranging for the translation of complete journals, aside from specialized services within the Government. It certainly is possible through our Division of Information Services and through the armed services with their large document accumulations, for any of our scientists to ask, in a variety of ways, for a translation for his purposes. (For him alone, if he wishes, anything he finds untranslated.) But the business of translating the Russian journals is getting to be quite massive.

Representative VAN ZANDT. What have we reached in the way of degrees Fahrenheit?

Dr. RUARK. This is one of the hardest things to talk about because so much depends upon the density of the plasma. Our people have a choice of working at high density, and then they come out with lower particle energies or temperatures. If we work with less dense plasmas—less material to heat—conditions are in some ways more favorable to the attainment of higher temperatures. So the figures run all the way from results with very dense plasmas at Los Alamos and here at Naval Research Laboratory, on the one hand, on over to the very tenuous plasmas at Oak Ridge. At Oak Ridge, the ions are put in at superthermonuclear temperatures. This means billions of degrees, while we need, for the ignition temperature of a deuterium-tritium mixture, 50,000,000°. So Oak Ridge is away up there, but the plasma is much too thin. There we must improve the density of the plasma.

On the other hand, at Los Alamos and Naval Research Laboratory, they have shock mirror machines, which are now operating at particle energies of the order of 15,000,000° to 20,000,000° centigrade. We should add 80 percent to get to the Fahrenheit numbers you requested. That means we have a factor of something like three or four to go on up, in order to reach the 50,000,000° centigrade, or say 90,000,000° Fahrenheit. In the last 3 years we have increased the energy of the dense plasmas and the middle-density plasmas in our laboratories, on the order of tenfold. We have a factor of three or four to go, in order to reach this ideal ignition temperature, which is not a particularly significant point, except that when we reach it we will know that we really are on our way to still better results.

Representative VAN ZANDT. Thank you.

Representative PRICE. Thank you, Doctor.

The next witness will be Dr. James L. Tuck, in connection with the Sherwood research program at Los Alamos. We have had Dr. Tuck before the committee on several occasions, mostly, however, in executive session. We are glad to have you with us, Doctor.

STATEMENT OF JAMES L. TUCK,[1] LOS ALAMOS SCIENTIFIC LABORATORY

Dr. TUCK. Thank you, Mr. Chairman.

Representative PRICE. You may proceed, Doctor.

Dr. TUCK. I have never been quite sure how to start these brief discussions, but I am going to talk, I suppose, for about 12 minutes, and I thought I would take a couple of minutes at the beginning to do a little recapitulating as to what we are up to.

Representative PRICE. What you are up to?

Dr. TUCK. What we are doing. I am being impudent enough to say not we, but what everybody is doing.

The first question one could ask is, are you quite sure there is any energy to be gotten? That is certainly true, because a hydrogen bomb that costs, whatever it is, gives you an awful lot more than X dollars worth of heat out. So you know the energy is there all right. On the other hand, I myself would regard it as a crazy way of getting heat to try to justify those things. So we want to try to find a better way of getting heat out.

One might ask, therefore, is it straightforward to get the energy out of deuterium, which we are all trying to do, by a controlled method, rather than an uncontrolled method? I can only say, in my opinion, it is far from clear yet whether we can do it. I mean there is no doubt that you can do it with bombs, but whether you can do it the other way is not clear. I am kind of optimistic and I think we will, but it is sort of doubtful just when.

The next argument that is frequently made, people say uranium is a good deal more common than people originally thought, and there is plenty of it around. What is the point of struggling along on this difficult, difficult line, when you could just as well use uranium, of which there is enough to keep everybody happy for maybe a thousand years.

I think myself that the arguments that are strongly in favor of controlled thermonuclear power are that the energy is nicer. I mean nuclear energy is not at best at times, but it is less unpleasant in the fusion form. There are no fission products. I think the big argument for the Sherwood project is no fission products. The fact that

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[1] Born: Manchester, England, Jan. 9, 1910; married, 1938; two children.
Universities: Victoria University of Manchester, bachelor of science, mathematics, physics, and chemistry, with honors 1931; master of science, physical chemistry, 1932; Dalton research scholar. Oxford University: Salter research fellow, M. A. Wadham College.
1939–42: Chief scientific assistant to Lord Cherwell on the Prime Minister's personal staff.
1942–43: Chief scientific adviser, Ministry of Defense.
1944: Awarded the Order of British Empire by King George VI for elucidation of the Munroe or shaped charge effect and for contribution to weapons. Requested by the U.S. Manhattan District for research at Los Alamos Scientific Laboratory in connection with development of the atomic bomb. Contributions to atomic bomb development included: (*a*) the explosive lens (with John von Neumann) used in fission weapons; (*b*) the urchin initiator (with Hans Bethe).
1946: First known paper on controlled thermonuclear reactions (with Stanislas Ulam); returned to Oxford, England, as supervisor of the Department of Advanced Studies.
1949: U.S. University of Chicago.
1950: Los Alamos Scientific Laboratory, research on the remeasurement of fusion cross sections with W. Arnold, J. A. Phillips, G. A. Sawyer, E. Stovall, Jr.
1951: Lecture on controlled thermonuclear reactions.
1952: Experiments began with Perhapsatron; became member of Sherwood steering committee.
1955: Obtained U.S. citizenship.
1956: Appointed associate division leader in charge of Sherwood research at Los Alamos Scientific Laboratory; present staff, 55.

the fuel is cheaper is irrelevant. It only costs 10 percent of the cost of power. So therefore I think that is the thing we should keep our eye on.

This business about there being no fission products is going to be very important someday. It is going to be very important someday because when fission reactors are as common as automobile engines, there will be such a lot of fission products dropped, no matter how careful you are, that life will get very unpleasant. If fission products were as common as exhaust gas we would feel very unhappy about it. It would be very nice to have a method which did not involve this.

Representative PRICE. Mr. Ramey would like to ask a question.

Mr. RAMEY. Isn't it true, though, with your thermonuclear reactions that you would very likely want to put a sheath of uranium around it, or something like that, and you would get your power out of it?

Dr. TUCK. I don't think so, myself. I think it is unnecessary. In other words, you could make life a little easier for yourself in the beginning stages by putting what is called a uranium multiplying blanket around your hypothetical reactor. You don't have to.

Mr. RAMEY. Some of the earlier reports seemed to think that this would be the most efficient way of getting power.

Dr. TUCK. I know it might be very efficient, but it also might be dirty, and you might rather want less efficiency and more cleanliness.

Mr. RAMEY. It would be a matter of cost.

Dr. TUCK. Yes. So therefore the reason we all want to do Sherwood is because of this cleanliness, I claim. So let us turn around to what we are doing. You might all say we started with sort of wild dreams and were in fact living at the time in a kind of fool's paradise. We all had hopes of making something, and set about doing it, and very soon we ran into snags.

Representative PRICE. I remember one time in testifying before the committee, one of the members asked you if we were about to get a breakthrough in this program, and your reply was that what you wanted was a break rather than a breakthrough. Are you still looking for the break?

Dr. TUCK. I have an idea one can see traces of getting a break, yes. I think there is a trace of such an indication around. You see I have been a pessimist from away back, but I am getting to be a bit of an optimist.

Representative VAN ZANDT. Doctor, could you give us any prediction now from the standpoint of years when we may have success? In other words, you are still cautious?

Dr. TUCK. No. Well, anyway, so there was the fool's paradise region which lasted from 1951–52 to about 1954 when hopes were properly put in their place, and it got very tough in 1954–55. Now, people are struggling against the difficulties they begin to appreciate are really there and now people are beginning to make what I call some real headway. In other words, they know what the trouble is.

To turn from the general, which was presumptuous on my part to talk about, to the particular, namely, Los Alamos work, we are now of the four major laboratories the smallest. We have 50 people. We are working on more or less lots of little gadgets and devices. Of our various gadgets we had four of them making neutrons of one sort or

another about 2 years ago, and we never knew whether any of them were going to be good, although one of them looked possible. It was a little device called Scylla, which made a very rapid compression by a magnetic field. We have had the equivalent of four professor level people working on this now since Geneva. We thought it looked successful at Geneva but we would not commit ourselves. We are now prepared to back our reputations that we have a thermonuclear reaction. It is not a very prolonged thermonuclear reaction. As of 6 months ago it lasted for 0.9 of a microsecond, and the temperature was, I think, a high H temperature, but not good enough to be of any value in a reactor. It lasts for 0.9 of a microsecond and the density of the deuterium is quite high.

You might well ask is this the breakthrough for a thermonuclear reactor, and our answer is a firm "No," it is not. It was not intended to be in the first place. It was intended to be an experiment to see if you could make a bit of a thermonuclear plasma to study and we have done it. It is telling us all nice things about the surfaces of stars. We are seeing things that have never been seen on the earth before. We have stellar lines which have never been seen on the earth before now. But I do not think this is in any sense of the word a breakthrough, or in any sense of the word the prototype of any kind of thermonuclear reactor. It is interesting. We are trying to build a longer and bigger one. We have had success in building a longer one. Once we made a quick squeeze and get the high temperature, we now have a quick squeeze and hold it. The Scylla I at Geneva did a quick squeeze which lasted 0.9 of a microsecond. The Scylla II does a quick squeeze and holds it for 7 microseconds. So we now have this temperature for 7 microseconds. You can see it on curves. The neutrons are produced and they stay being produced for a while. As a matter of fact, when you go into the physics of this particular way of doing things, you find that it is really very unlikely to lead to any kind of profitable power.

This is the end of Scylla, as far as I am concerned. It is an experiment. We think it is a very successful experiment, and that is what it is.

Now, then, we have other fish to fry. We have been playing with other ideas and one of these in particular is a thing that has great hydromagnetic stability. It has various names. We call it picket fence. More generally throughout the world it is known as a cusped geometry. It is a magnetic field with magnetic lines curved away from the plasma. This was thought up years ago, but at the time who was going to bother with that? It was a very leaky way of doing things and there were better ways. Everybody agreed there were better things to be done. The other things have gotten shaky one way or another, and this thing is looking a little relatively better than it used to do. But we never had any decent way of putting any plasma inside it. This is where our new idea seems to be paying off. We call it entropy trapping, and the idea is to take a burst of plasma from a projector which we have designed, which is so strong that it can force its way through the magnetic field, slip inside, and by the nature of changing it from a shot into a spray, it sticks once it is in. This is a very pretty simple concept. But when we came to try it in practice,

it was a good deal more complicated than we expected. Nevertheless, we have indeed achieved considerable buildups of plasma this way.

The thing that was more tricky than we expected——

Mr. RAMEY. Could you describe the tube and your cusp effect that you used for this device?

Dr. TUCK. You want me to do it on the blackboard, do you mean?

Mr. RAMEY. Isn't it sort of just a tube and then it elongates?

Dr. TUCK. No. I have to have a blackboard, I am afraid, for this.

Mr. RAMEY. Perhaps you can do it on the back of that chart.

Dr. TUCK. The cusp geometry is fairly obvious. It is this shape [drawing on board]. These are the magnetic field lines. This is the place we want to put the plasma in if we can. Now, the gun looks like all guns. It has a long barrel with a muzzle and a butt, and we can load this thing with gas and fire a jet of plasma. The trick that we like about entropy trapping is that this is such a big burst that it can drill its way through this thing, open this up, get inside, and then this closes again. It was a little more tricky than at first sight appeared. The paper that we wrote made all sorts of remarks about a certain density here, and a certain magnetic field there. It proved to be a little more complicated. What it does, if this is the amount of opening, and this is the amount of magnetic field, instead of it being a sudden cutoff, bing, like this, it is a gradual thing. In fact, this jet drills its way as a thin stream through the middle. We have actually achieved high density plasma in this object for 30 microseconds which is a lot longer than we have ever had anything before.

I think this idea has interesting possibilities but that is as far as I am prepared to go. In fact, I have never been much of an optimist for making thermonuclear reactors, but I think I can faintly see that there is a possibility, and I think that is as far as I think anybody dares to go.

Mr. RAMEY. Were the Russians interested in this injector device?

Dr. TUCK. They seemed to be very interested and they said they would try an OGRA. I must admit that they have a very nice machine to try it on.

Mr. RAMEY. You mean a large machine, and if they had a large injector, it might work?

Dr. TUCK. This idea will work on almost any machine. It is just a trick of getting things through magnetic fields. It will work on OGRA, a mirror machine, just as well as on a picket fence. I think that is all I have.

Representative PRICE. Doctor, you have had quite a bit of contact with the Russian group interested in the thermonuclear program when they were out at Los Alamos and so forth. In your discussion with them and the areas in which they were interested, did they indicate that they were pretty well up with the rest of the world in this program?

Dr. TUCK. Did they indicate or did I think?

Representative PRICE. Did they from the type of information they sought from you and their reaction to what you were doing.

Dr. TUCK. Their scientists can understand the subject as quick as a flash, just as any other scientist.

Representative PRICE. I recognize that fact. As a practical matter, did they indicate that they were thinking in the same line as our scientists were in this particular area?

Dr. TUCK. I think if you are prepared to make certain exceptions of detail, you will find that everybody has thought of the identical things. For instance, I think our investment in stellarators is much larger than they went in for. But taking it by and large, everybody more or less proposed the same generalities. Smaller subcontracts of these ideas—for instance, so far as I know they had not thought of entropy trapping, but certainly had been working on picket fences—it is just a matter of time.

Representative PRICE. Are there any questions of Dr. Tuck? If not, thank you very much, Doctor. We are glad to have you here.

(Dr. Tuck's summary of the present Sherwood situation follows:)

SUMMARY OF THE PRESENT SHERWOOD SITUATION AT LOS ALAMOS[1]

(By James L. Tuck, University of California, Los Alamos Scientific Laboratory, Los Alamos, N. Mex.)

Continuing from the time of the Atoms for Peace Conference in Geneva in September 1958: At that time we had 50 people at Los Alamos working on Sherwood and were spending about $3.5 million per annum. Our main line of research was on the pinch effect and we had Perhapsatron S–4, a toroidal stabilized pinch operating (exhibited at Geneva) and numerous straight pinches or Columbus-type machines. Our subsidiary effort included a plasma confined by rotation in a magnetic mirror geometry (Ixion) and last but not least, a rapid rising small magnetic mirror system called Scylla, also exhibited at Geneva. Four or five of these different devices were producing neutrons, and one of these devices, Scylla, even looked as if the neutrons might be thermonuclear. At the time of Geneva, we had to resist very forcibly any imputations from the press that Scylla was thermonuclear. We just did not have enough information.

Turning to the present time, we still have 50 people working at Los Alamos on Sherwood. By the end of the year, we shall have spent about $3 million, which is about a million less than we expected so we have had to return it. We have now accumulated thousands of measurements of many different kinds on the Scylla machine and the press can now impute as much as they like, for we really think it is thermonuclear now.

We seem to have a plasma of hot deuterium in the form of a small egg-shaped fireball about 2 centimeters in diameter, containing about 5×10^{15} deuterons/cubic centimeter with a temperature of 1.3 ± 0.1 kilo electron volts. The electrons have a temperature of about 240 electron volts and the fireball lasts about nine-tenths of a microsecond in the original Scylla I apparatus and in this time it emits about 10 million thermonuclear neutrons.

The next stage of this apparatus—Scylla II—consists in essence of the original Scylla I, using a quick magnetic squeeze followed by a slow second squeeze. This has had the very satisfactory effect of extending the duration of the neutrons to about 7 microseconds.

We shall probably build a bigger, slower Scylla operated from our new big condenser bank, called Zeus, but in the meantime we have a lot of studying to do with Scylla II. We do not quite understand how the initial heating of the plasma takes place before the quick squeeze raises this temperature to thermonuclear heights. About 6 months ago, we took the major step of narrowing down on what was our major activity, the pinch effect, to make way for a promising looking newcomer—high energy injection into a picket fence magnetic system. The picket fence or cusped geometry is not a newcomer, having been proposed in 1955. The new angle is a method of injecting it full of hot plasma by what we call entropy trapping. In this, we take an intense jet of plasma from a previously developed plasma gun and project this at the axial cusp of a picket

[1] Work performed under the auspices of the U.S. Atomic Energy Commission.

fence. The jet is intended to drill through the magnetic field and fill the interior of the hollow picket fence. It is predicted that the process of drilling through the magnetic field will cause the jet to become diffused or disordered so that it has lost penetrating power and is unable to drill its way out again on the far side. The experiment is now our top priority operation and while proving to be a little more complicated than was envisioned in the beginning, is looking rather hopeful. We can certainly keep plasma inside the picket fence long after the gun has been turned off. The main surprise that has come out of the experiment so far has been that the opening and closing of the magnetic field turns out to be more gradual than expected. We now have an urgent need for guns that will provide a good clean jet of plasma that already has the energy we need to produce thermonuclear reactions and we therefore are turning a substantial part of our effort into understanding the plasma gun we now have and developing new ones.

If the entropy trapping into the picket fence works out and fulfills our expectations, then we are ready with a geometry by which the cusps are closed on themselves and the whole system wrapped up into a torus. This geometry, the caulked picket fence, is not new, having been proposed by C. L. Longmire and the writer in 1957, but what is new is a levitated method of construction which may have the effect of stopping up the last few holes in it. The levitated caulked picket fence is a complicated device and we certainly shall not build one until we have more results from our simple picket fence studies. In conclusion, I can only say that for the first time in all of Sherwood, in this device I see faint glimmerings of a possibility of making a thermonuclear reactor.

Representative PRICE. The next witness is Dr. Richard F. Post of the Lawrence Radiation Laboratory.

Doctor, I understand that you are scheduled to discuss several projects.

STATEMENT OF RICHARD F. POST,[1] OF THE LAWRENCE RADIATION LABORATORY

Dr. POST. In keeping with the spirit of these "Frontiers" hearings, I am not going to attempt to give you a detailed picture of our Sherwood research at the Lawrence Radiation Laboratory, but am instead going to hit some selected high points, to give you a feeling for our approaches to the problem, and to tell you of some recent important experimental results. Finally, I will express some personal opinions about where we are going and what it means for Sherwood research in the future.

The program of the Lawrence Radiation Laboratory is carried out both at the Berkeley and Livermore branches of the Laboratory. As you will see, it is carried out on a broad basis, and encompasses a wide variety of experiments, a philosophy which we have found has many advantages. There are approximately 60 scientists involved in the total program, 10 in Berkeley and 50 in Livermore, together with a ratio of about 3 scientific support personnel (that is, engineers, laboratory technicians or shop personnel) for every scientist.

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[1] Born: Pomona, Calif., Nov. 14, 1918; married 1949; 3 children.
Education: Bachelor of arts in physics and mathematics, Pomona, Calif., 1940; doctor of philosophy, physics, Stanford University, 1950.
Professional experience: Instructor in physics, Pomona College, 1941; physicist, Naval Research Laboratory, Washington, D.C. (working on the problems of propagation of underwater sound, mine countermeasures, submarine sonar, 1942–45, particle accelerator, 1945–46; research associate in physics, Stanford, 1947–52; project leader, University of California Radiation Laboratory, controlled fusion research. In charge of one of the major subgroups investigating various approaches to the problem, 1952 to present.
Societies: American Physical Society.
Major fields of research: Electron physics; traveling wave electron linear accelerators; ultimate resolving time of scintillation counters.

The Berkeley effort, which is under the scientific direction of William Baker and Wulf Kunkel, accounts for about 15 percent of the total effort, and is directed toward several small-scale experiments on fundamental plasma diffusion and rotation effects. Many of the early experiments on the pinch effect were also carried out in Berkeley. In addition to the full-time Laboratory staff, it employs five graduate students, who are carrying out their thesis work, and five visitors from foreign countries who are aiding in the investigations. As an example of their experiments, they recently published an excellent paper on their observations of the generation and propagation of a special class of waves through a plasma in a magnetic field. For further details of their work, may I refer you to the supplementary material which we have submitted for these hearings. (See additional information on the Livermore program, p. 119.)

The remaining 85 percent of the Laboratory's effort is carried out at Livermore, where there are three major experimental groups. Although following different approaches, all three of these groups have a present common purpose—that is, studying problems of confinement and stability—which we feel are the crucial questions for the future of Sherwood. These three groups are the Pinch Research Group, under the scientific direction of Stirling Colgate, the Astron Group, under the direction of Nicholas Christofilos, and the Mirror Machine Group under my direction.

Before describing the research in progress in our various groups, I would like to amplify my remark on the importance which we attach to the study of plasma stability and long-time confinement. Not so very long ago, it was widely thought that the observed poor confinement of plasmas in magnetic bottles could be entirely ascribed to "hydromagnetic" instabilities—that is, instabilities where the plasma pushes aside or "burbles" between the magnetic lines. Edward Teller has likened this kind of instability to what would happen if you tried to hold lukewarm jello in a cage made of paraliel-lying rubber bands. Here the plasma is, of course, the jello, and the magnetic lines are the rubber bands. However, as long as we were thinking only of this kind of instability, the actual behavior of the plasmas in many experiments—not just our own, but in most other laboratories—was most puzzling. Then the realization came that there are actually many other kinds of instabilities which can occur in a confined plamsa, ones which may occur even in situations where the plasma is not subject to simple hydromagnetic instabilities. These instabilities were predicted theoretically to arise if certain flowing motions or other special motions of the plasma were present. They would be expected to give rise to turbulence in the plasma, just as water flowing through a pipe at too high a velocity may break into turbulent flow and behave in an entirely different manner from the way it behaves in its normal flow. You may have seen another example of such instabilities if you have seen the nozzle end of a high-pressure firehose whip violently back and forth when the flow through the nozzle becomes too great. Flow or velocity-space instabilities would make themselves known by greatly increasing the rate of diffusion of the plasma across the magnetic field, or by greatly increasing its electrical resistance. Either effect would interfere seriously with obtaining the long-time confinement of a plasma.

One might think that the theoretical prediction of unsuspected new classes of plasma instabilities would represent another barrier to our future progress. Quite the contrary is true. If we are armed with a complete, or nearly complete, catalog of the various ways in which plasmas can become unstable and know theoretically the physical conditions necessary for these instabilities to occur, then we can begin to make real progress toward eliminating them. This is because in looking back over our experiments we can now make sense where there was chaos before. Furthermore, we can now begin deliberately to develop critical experiments aimed at understanding, and then avoiding each instability, one by one, until hopefully, we have, by luck or good design, eliminated them all. This philosophy, of critical experiments aimed at understanding plasma stability, has been pursued actively in our Livermore program for some years. We feel unanimously that this goal is much more relevant to the future course of Sherwood than any other we might have pursued, for example, the momentary achievement of a small volume of plasma heated to the lower edge of thermonuclear temperatures.

I have taken the time to outline this common denominator of the various parts of our program, since it is helpful in understanding what we are trying to do, and because I feel this approach has paid off handsomely and will continue to do so. In particular, we feel that these studies have already turned up a promising new approach to plasma confinement (in the Pinch program) and in the Mirror Machine program, have led to the demonstration of an absolutely stably confined hot plasma—the first and only such case in Sherwood that I know of. Experimental evidence is also now being gathered in the Astron program which can be expected to shed further light on the question of plasma stability, in this case, in the novel magnetic confinement geometry which has been proposed by Nicholas Christofilos.

I would like now to give a thumbnail sketch of each of the Livermore programs and then conclude the discussion by some remarks which will summarize some of my own views on the future of Sherwood.

PINCH RESEARCH

The pinch research group accounts for about 25 percent of the Livermore Sherwood effort. This group has been particularly effective in elucidating the origin of the neutrons which have appeared so regularly in nearly all pinch experiments. You may remember the testimony, in 1958, by Stirling Colgate, where this matter was discussed. At that time Colgate expressed the opinion, which was not widely accepted at that time, that all neutrons emitted from the various toroidal pinch machines (such as the British ZETA, or its U.S. counterparts) were of nonthermonuclear origin, and were in fact external evidences of fundamental instabilities present in the plasmas of all of the so-called stabilized pinches. This opinion, which is now generally accepted, led Colgate's group into a critical study of pinch instabilities, and to an abandonment of the simple stabilized pinch as a promising confinement geometry. A similar conclusion has been reached at Los Alamos, where the effort has largely shifted to confine-

ment geometries other than the pinch. However, at Livermore the critical study of the pinch led to modifying it rather than abandoning it. It was predicted theoretically that many of the desirable confinement properties of the toroidal pinch—that is, compactness, high efficiency of utilization of the magnetic field energy, and at least theoretically, excellent confinement time, could be retained if the central portion of the old stabilized pinch were to be replaced with a solid conductor, which would carry a portion of the pinch current. In this case, the plasma would be confined between the magnetic field produced by the central conductor, and an externally imposed magnetic field generated by coils wound around the doughnut-shaped confinement chamber.

Theoretically, such a configuration can be arranged to inhibit all hydromagnetic instabilities, and indeed Colgate, and Harold Furth have found experimentally that the gross stability properties of this kind of a hard-core pinch, as they call it, are very much better than those of the old stabilized pinch. The more easily detectable effects of the flow-type instability have also been suppressed experimentally; but here it is not yet known whether the improvement is going to be sufficient to allow the completely stable containment of a hot plasma. There are certain special types of hard-core pinches for which the theoretical outlook against all types of instabilities is very good.

You may be wondering how one would support a solid conductor in such a way as to thread around the inside of the doughnut chamber, and to cause it to carry a heavy current. Material supports or wires connected to the outside world are not usable, as any object passing through the plasma would drastically interfere with the confinement. However, one should remember that pinch confinement is essentially transient confinement—not a steady-state process of the kind visualized for the Stellarator or the Astron. For confinement studies, therefore, and possibly even for practical application, an available confinement time of a small fraction of a second should be sufficient. It therefore becomes practical momentarily to magnetically levitate the hard-core (which is a heavy copper ring) in the chamber, and then to induce a heavy current in it by transformer action, in the same way as the current was induced into the plasma in the older toroidal pinches. Colgate calls such a hard-core device a Levitron. Small experiments which prove the feasibility of the magnetic levitation have been performed, and a medium-sized Levitron (12-inch bore) is now being readied for tests of plasma confinement and stability. As far as we know the investigation of the Levitron concept is unique to Livermore, and we feel that it is a promising way of investigating plasma confinement in a closed geometry (that is, a doughnut). It is worth noting that any light that can be shed on plasma confinement in the Levitron may have a substantial bearing on the better understanding of confinement in a Stellarator, where a similar closed torus is used, albeit minus the central conductor.

Stirling Colgate and Harold Furth have contributed a portion of the supplementary material which we are submitting on the Lawrence Radiation Laboratory program, and I would like to refer you to it for additional details on the Levitron and other of their experiments.

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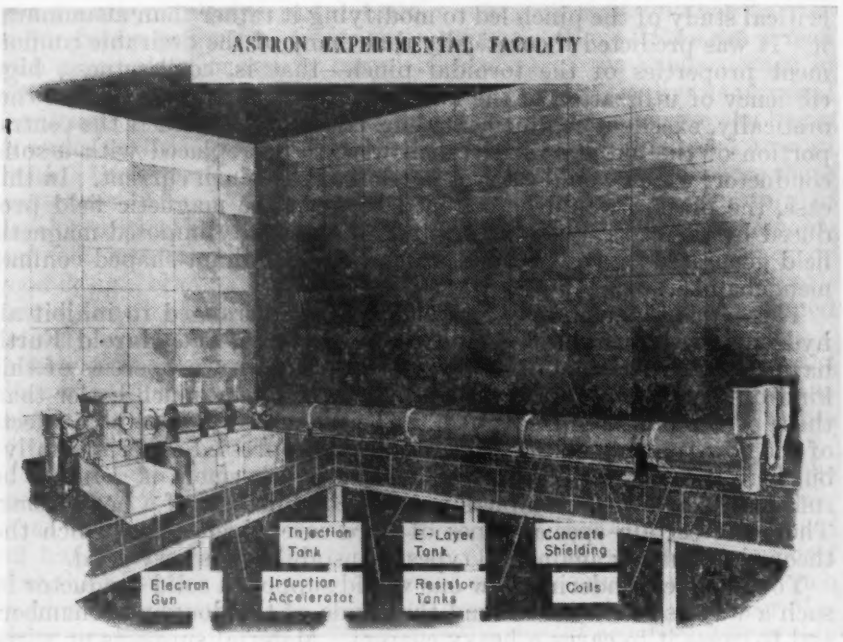
CHART 1



THE ASTRON PROGRAM

The Astron program also accounts for approximately 25 percent of the Livermore effort. Although the Astron concept was proposed as early as 1953, the theoretical aspects were poorly understood at that time. Consequently it required 3 years of theoretical work before the Radiation Laboratory and the Commission decided, late in 1956, that an experimental program was warranted. The experimental work to date has been preparatory in nature.

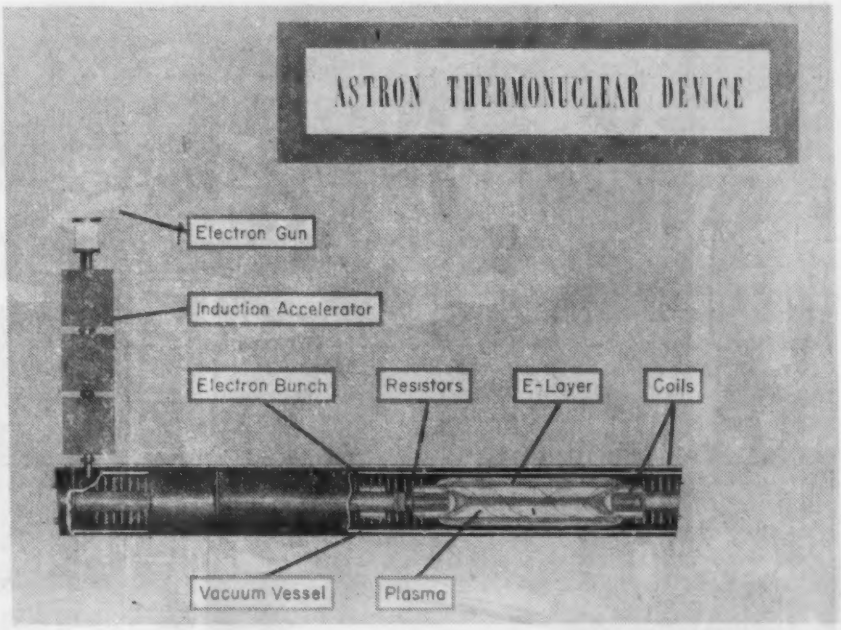
The key feature of the Astron is the use of a layer of very high energy electrons which is to be established within a long cylindrical evacuated chamber. The electrons of the layer, which must have an energy of at least a few million electron volts, rotate around the axis of the chamber under the influence of an external magnetic field and thereby form a current sheath, so that one is, in effect, winding a magnet coil with high energy electrons. This is illustrated in diagram appearing on page 103. If a sufficient number of these rotating electrons can be trapped within the external field, these electrons will themselves generate a strong field throughout the entire region inside the layer. Since this strong internal field will be opposite in direction to the external applied field which holds the electrons, it can cancel and even reverse the magnetic field direction. The resultant field, that is, the sum of the externally applied one and the field from the electron layer, will then form a closed pattern of magnetic lines which passes through the layer itself. This pattern of lines forms the confining field. Since each magnetic line closes on itself, charged particles trapped in the field can in principle only escape by slowly diffusing across the magnetic lines.

Mr. RAMEY. Then you have a plasma inside that is heated up?

Dr. POST. Yes. I have just described the first phase, which is the generation of the confining field.

In the Astron the layer of high energy electrons, called the E-layer, must perform another and very important function, that of ionization and heating. Assuming that a stable layer can be set up and held in the evacuated confinement chamber, then in order to build up a hot confined plasma, neutral gas would be slowly admitted to the chamber. Upon encountering the E-layer electrons this gas would become ionized, forming a trapped, but low temperature, plasma.

CHART 2



Collisions between the E-layer electrons and the plasma electrons would result in heating the plasma electrons, and then the plasma ions, up to a high temperature. To maintain this process in a steady state it would be necessary to continuously replenish the E-layer electrons and to inject new gas.

For the Astron to function, it is necessary both that the E-layer can be formed and is stable by itself, and in addition, that the confining field generated by the E-layer be capable of confining dense high-temperature plasma. However, it should be noted that the E-layer is a concept not encountered in any other of the Sherwood approaches. Consequently, the mere presence of this layer, although necessary for achieving the final purpose of the Astron, creates additional theoretical and experimental problems not encountered anywhere else in the Sherwood program. Furthermore, the novelty of the E-layer idea implies that previous experience in other parts of the Sherwood program is not particularly helpful in the development of the components required for the Astron program. Much of the Astron de-

velopment, therefore, is in areas which are truly "frontier" in nature. These technological developments are now proceeding, in preparation for a step-by-step test of the Astron principles.

The photograph below (chart 3) shows equipment which has already been set up to investigate some of the injection problems. An example of these developments is the electron accelerator which is the injector used in the production of the E-layer. This accelerator must provide a pulsed beam of 3 to 5 Mev. electrons at the very high current of 200 amperes. The basic design for this accelerator which involves several novel approaches, has now been worked out and the procurement of parts is underway.

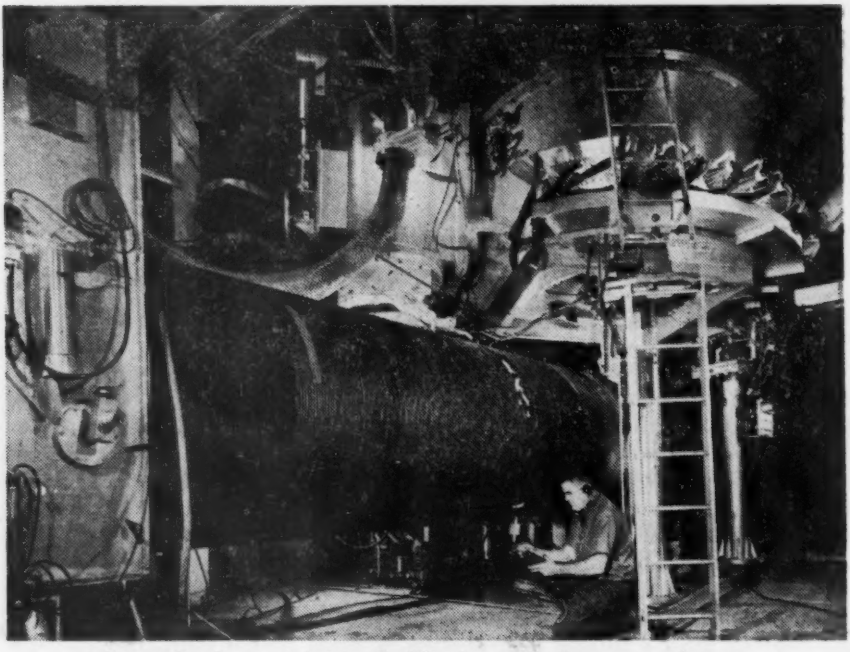


CHART 3.—Present Astron facility for preliminary experiments.

The very nature of the Astron suggests that its study should be carried out in a step-by-step fashion on a single piece of experimental apparatus. This is in contrast to the rest of our program, where it is more readily possible to set up many small, separate experiments. Thus, the Astron program will employ a single facility, which is shown in schematic form in the next chart. This facility (which should be ready by the spring of 1961) is to be used for investigation of the following questions:

1. Injection and trapping of high-energy, high-current electron beam within a properly shaped magnetic field.

2. The possibility of creating a stable E-layer by trapping and holding many electron bunches, particularly the critical question of the stability of this layer and whether or not a closed pattern of lines can be created.

3. Plasma confinement and heating by the E-layer once it is established, including the limits on the amount of plasma which can be

held, and its rate of diffusion or escape across the fields. It appears highly possible that if the E-layer itself is stable, it will be capable of supporting some plasma pressure, so that at the very least a low-density plasma could be confined. Whether or not a substantial plasma pressure, enough to be of practical interest, could be held would have to be experimentally determined.

Representative PRICE. Would you explain that chart?

Dr. POST. The diameter is of the order of 4 feet and the length overall is about 30 feet.

Representative PRICE. In what you have described there is the E-layer tank about 30 feet long?

Mr. CHRISTOFILOS. It is 20 feet.

Dr. POST. The most crucial of these questions is the stability of the E-layer itself. This problem is extremely difficult to treat theoretically, so that one must count heavily on experimental investigation of this question.

Although physically sizable, the Astron model itself should be neither particularly difficult to construct nor particularly expensive. This is because the magnetic fields to be used in this experiment are relatively low, and the vacuum system is of simple construction. The most expensive part of the facility is the electron accelerator, to which I have already referred, but because it is a pulsed accelerator, it appears that it will be no more expensive than more conventional electron accelerators of comparable energy. One might note that preliminary engineering studies have shown that if the present Astron concept is sound, a working reactor would not be expected to be much larger physically than the present test model, although it would require much more massive field coils and a much larger accelerator.

We feel that the Astron principle, which is a truly novel idea in the Sherwood field, and which is not being elsewhere pursued, contains some features which are well worthy of investigation. The basic question, that is the stability of the E-layer, should be answerable in a relatively few years. If a stable, field-reversing E-layer can be produced, Christofilos will have introduced a very important contender in the search for controlled fusion.

For additional information on the Astron, may I refer you to the supplementary material.

THE MIRROR MACHINE PROGRAM

The mirror program accounts for the balance of the Livermore Sherwood effort, and was the first approach to be initiated at the radiation laboratory. It might be pointed out that Enrico Fermi is in a sense the originator of the mirror machine, since he worked out a theory of the origin of cosmic rays which depended on the magnetic mirror principle, at about the time we became interested in high-temperature plasmas. The possible application of the magnetic mirror principle to the confinement of a high-temperature plasma was first considered at the radiation laboratory in 1952. Within a year the basic idea was shown to be sound and by 1954 it had evolved into a specific approach to the fusion problem which has come to be called the mirror machine. Although for many years my group at the Livermore Laboratory was the only one actively carrying out

magnetic mirror research, in recent years the idea has been taken by many other laboratories so that it represents one of the most actively pursued approaches.

Our effort in the mirror group is divided among five subgroups, four of which are concerned with various aspects of the physics of injection, heating, and confinement of plasmas in a mirror machine, the fifth, and newest group, is investigating a problem of technology which we consider potentially very important and to which I will refer at the end of my presentation.

The next chart (chart 4) shows the schematic arrangement of coils, vacuum chamber, and magnetic lines in a simple mirror machine. As you see, confinement is provided entirely by externally generated magnetic fields, over which we have complete control. In many of our experiments the mirror fields are used to compress and to manipu-

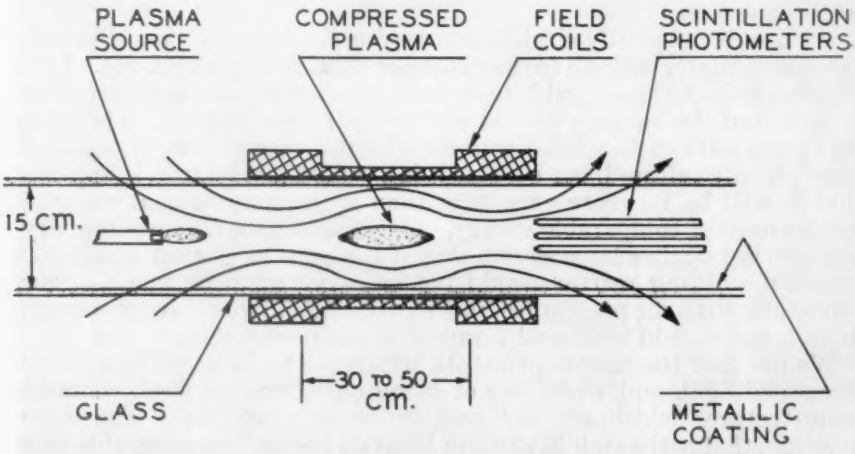


CHART 4.—Schematic arrangement of magnetic mirror machine.

late the plasma in order to heat it and to study its confinement. Provided these manipulations are carried out slowly we can predict theoretically what should occur with considerable accuracy.

We have just recently completed a series of measurements on our oldest and simplest mirror machine—which we call Table Top—that I would like to describe briefly. As I mentioned earlier, we feel that this work represents the first and, as far as we know, the only documented case of completely stable confinement of a hot plasma. There seems to be no doubt about the general validity of the result (since the plasma really stays confined, and for a long time), but we do not claim that the results are general—they may be only a special case. What is important is that this one experiment shows that it is indeed possible to confine a hot plasma stably in a magnetic field—a situation which some people were beginning to doubt could ever be reached.

In the Table Top experiments our heated and confined plasma is produced by injecting a small burst of low-temperature plasma into the evacuated confinement region while the magnetic field is very low, and then increasing the field slowly so as to compress and heat the plasma. Several years ago we found that we could heat and hold

a plasma confined by this process, and we reported our results at the U.N. 1958, Geneva Atoms-for-Peace Conference. However, to show that a plasma is absolutely stably confined you must do more than simply demonstrate some confinement. You must show not only that it is held for a time very much longer than the theoretical time in which instabilities should develop, but also that the particles escape across the magnetic field and out of the bottle at a rate which is consistent with the very slow theoretical leakage rate that any magnetic bottle should exhibit. Furthermore, the significance of the experiment is greater if the confined particles represent an "interesting" plasma—that is one in which the particle density is comparable to that required in a fusion reactor. Also, at least one of the charged-

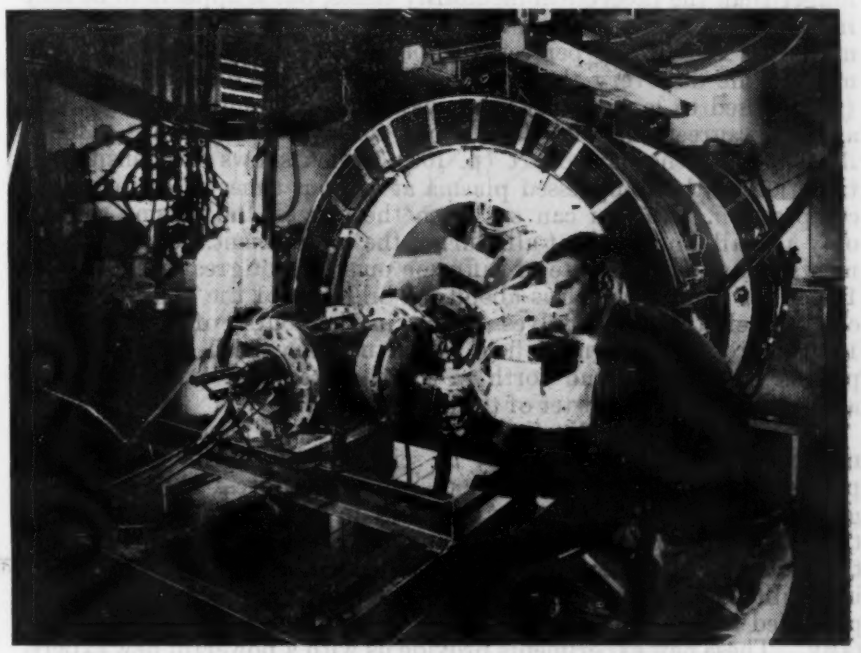


CHART 5.—Table Top high compression magnetic mirror experiment.

particle components of the plasma (electrons or ions) should have a temperature in the thermonuclear range, and the plasma should have dimensions which are at least several particle-orbit diameters thick, so that it looks like a confined gas, and not like a group of particles moving in a highly organized way.

Since Geneva our measurements on Table Top have resulted in much more accurate determinations of the density and temperature of the plasma, and of the rate of escape of the particles. We have found the particle densities to be about those which would be required for a self-sustaining reactor, and the electron temperature has been measured at some 400 million degrees, which is well into the thermonuclear range. Because of the injection method we used the ions did not get as hot, certainly not over some 10 million degrees.

Representative PRICE. How long do these temperatures last?

Dr. Post. Confinement has been observed up to 30 milliseconds in these experiments.

Let me emphasize, however, at this point that since we were not looking for thermonuclear reactions, but for a provable case of stable confinement, the fact that the electrons rather than the ions possess the highest temperature is not particularly important, just as one could in principle demonstrate the aerodynamic principles of aircraft flight in a wind tunnel just as well by using carbon dioxide as by using air. The result would be a special case, but everyone would still believe that flight was possible.

What we have found in these studies is that, within the accuracy of the measurements, not only is our plasma confined some 100,000 times longer than the theoretical instability times, but it appears to be leaking out only slowly, and at a rate which agrees with the theoretical minimum rates for a perfectly stable plasma. What encourages us most is the discovery that the rate of leakage across the field lines (as opposed to leakage out the ends) is almost unmeasurably small, again in agreement with the theory for a stable plasma. This is illustrated in the next chart (p. 109), which shows the density distribution of the compressed plasma at various times after the initial compression. As you can see from the curves, the plasma diffuses only a small amount, if at all, during the confinement. Our measurements show that this rate of diffusion must be not greater than about 1 foot per second, which is about one-billionth of the velocity of the confined electrons themselves. Thus in the course of diffusing out 1 inch across the magnetic field in our machine, each electron in the plasma moves back and forth in the chamber over a total distance about equal to the diameter of the earth.

Since our recent publication of these results, we have obtained some new data that we consider may be scientifically of almost equal importance. We have found it possible to compress the plasma so greatly and crowd the particles so closely in the middle of the machine that the plasma becomes unstable and begins to diffuse rapidly across the field. What delights us is that we can induce or eliminate this instability at will, by controlling the plasma conditions, and that we predicted its existence theoretically before we produced it experimentally. These new experiments provide us with a powerful new experimental tool by which to investigate the laws of stability of a plasma, since we can venture into instability land as far as we like, but we can always come back and touch base when we feel the need.

Encouraging as these various experiments are to us, it is just as important to be aware of what they do not demonstrate as to understand what they do prove. They are not thermonuclear experiments, since the ions would have to be several times hotter to produce thermonuclear reactions; they are carried out with a volume of plasma much smaller than that required for a thermonuclear reactor; they still are subject to appreciable experimental inaccuracies; and they are conducted under conditions where the electrons are hotter than the ions, a fact that might be important for subtle reasons of which we are not aware. We are therefore working very hard in other parts of the Mirror program to remove these restrictions, and to explore plasma stability under more general circumstances, where the ions should be much hotter than the electrons, and should possess tem-

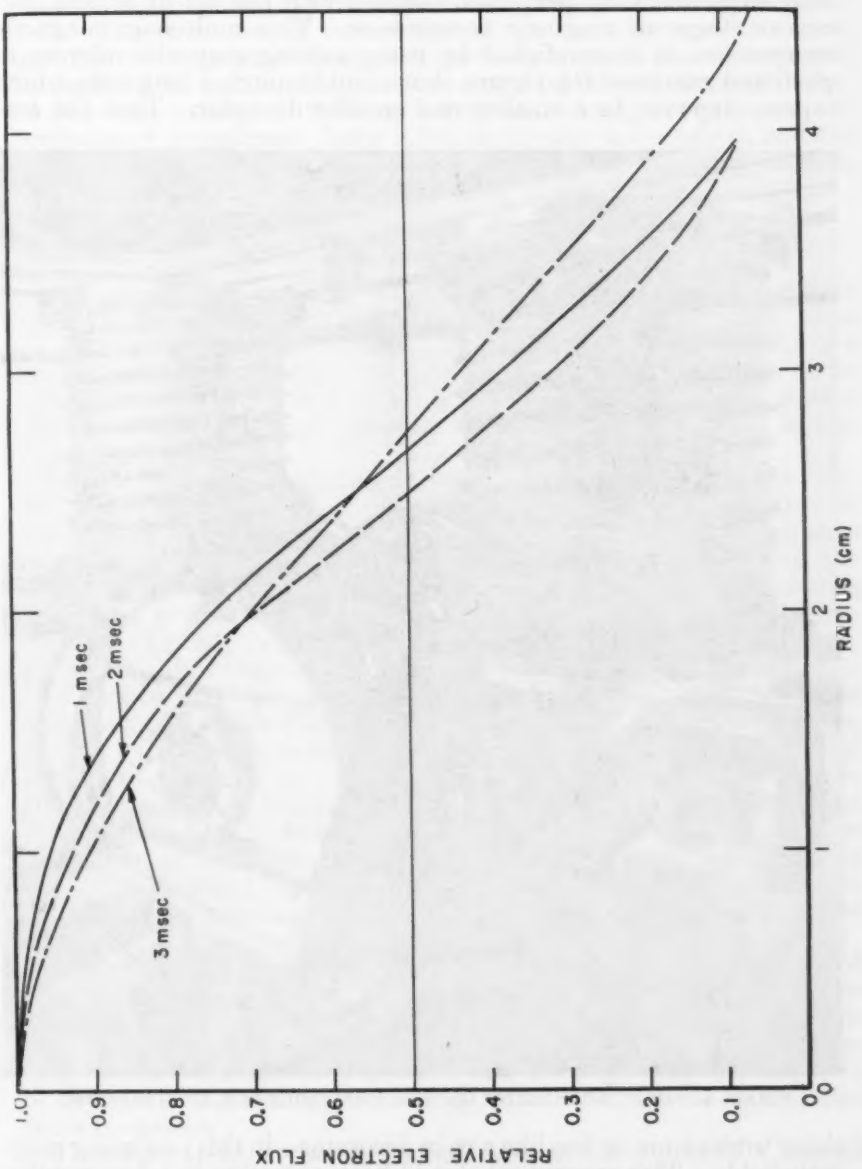


CHART 6.—Normalized radial flux distribution.

peratures in the thermonuclear range. Time does not permit describing these experiments, so I will only say that one of them employs some new methods of high energy plasma injection which we have experimentally perfected, coupled with the use of several successive stages of magnetic compression. This multistage magnetic compression is accomplished by using moving magnetic mirrors to push and compress the plasma down and through a long tube which tapers, stepwise, to a smaller and smaller diameter. Thus the ma-

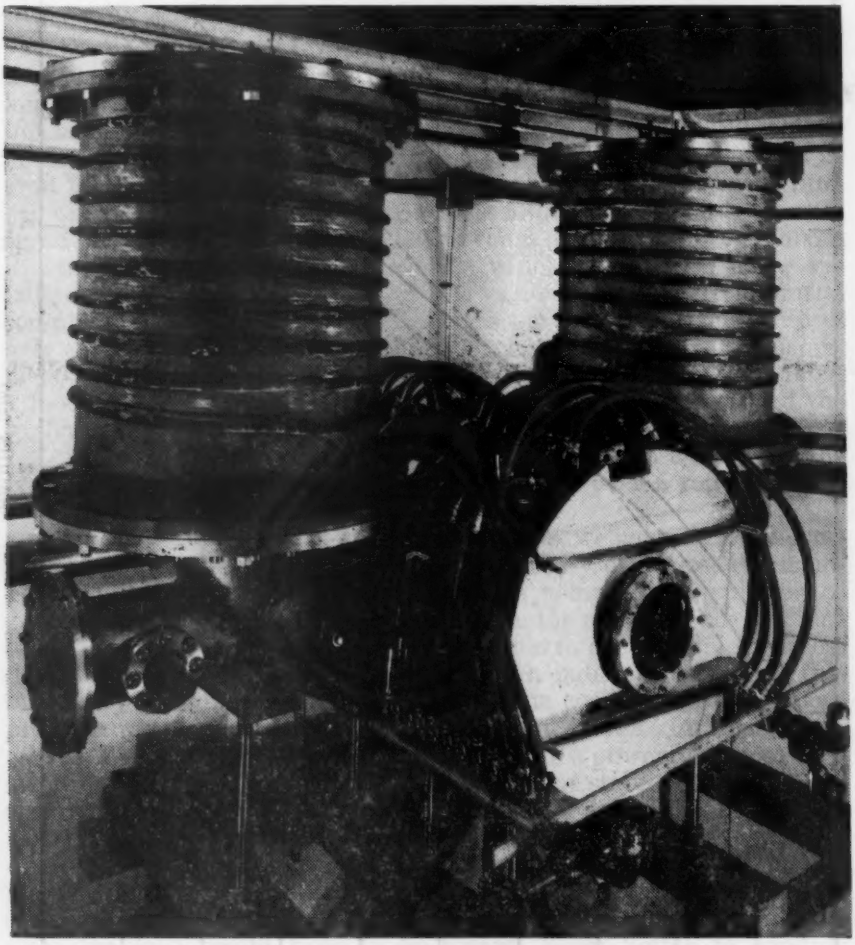


CHART 7.—Baby Alice mirror machine comprising fast atom injection.

chine works more or less like a cake decorator—in this case using magnetic fields. This experiment should be in operation in a few months, and we are anxiously waiting to see what we will find. I will make no guarantees at this time.

Another approach which we are making to study confinement, is our Alice experient (chart 7), also now under construction. This experiment is relatively small in size, and is exceedingly simple in

concept. But it involves some very difficult technology. In Alice, we will try to create a hot plasma merely by shooting a beam of fast atoms across a highly evacuated chamber. This chamber is surrounded by magnetic mirror coils which produce a steady magnetic field. The ionization of even a few of the fast atoms, as they pass through the chamber, will cause them to be trapped and in turn will be expected to lead to a cumulative trapping of still more particles. In this way we hope to build up a very hot, steady-state plasma. A preliminary experiment, Baby Alice, is already in operation, and the main experiment should be ready before the end of this year. Again, I prefer to make no predictions as to what we will find.

CRYOGENIC COILS

In concluding I would like to present a brief outline of the fifth and newest component of the Mirror program, and to explain what its eventual significance may be. In this program we are making a serious attempt to bring a very old idea to practical reality. The idea is that it should be possible greatly to reduce the huge powers needed to generate large volumes of intense magnetic fields by the following means:

It is well known that the electrical resistance of a metallic conductor becomes less if it is cooled to a low temperature—I am not here talking about the phenomenon of superconductivity, which cannot exist in the presence of a high magnetic field. Reducing the electrical resistance of the conductors in a magnet coil will, of course, reduce the power required to produce a given magnetic field. However, we can only hope to save in the overall energy cost of generation of a high magnetic field if the power required to energize a refrigerated coil plus that required to run the refrigeration plant itself is less than that to run an ordinary coil at room temperature. Now at very low temperatures, say 20° above absolute zero, the usual factor which limits the drop in electrical resistance which can be attained by cooling is simply the purity of the conductor. This must be very high to achieve a low resistance. Thus if we had set out, as recently as 10 years ago, to calculate the improvement achievable by refrigeration using the data then available, we would have no doubt been led to the pessimistic conclusion that little, if any, gain would be possible.

However, the last decade has seen a revolution in the technology of superpure metals, in the physics of electrical conductivity at low temperatures, and particularly in the technology, the size and the efficiency of cryogenic equipment—that is, ultralow temperature refrigerators. We can trace this last fact directly to the development of cryogenic engineering technology, stimulated by the missile and space programs.

Therefore about a year and a half ago, in the course of a general study of the minimum size of a mirror machine which I was carrying out, when the question of refrigerated coils was again examined, I was delighted to discover that the theoretical calculations indicated that a large reduction in both power requirements and in dollar cost of high magnetic fields could be expected. It appeared that either very high purity aluminum, or high purity sodium metal encapsu-

lated in stainless steel tubes, should make very good conductors from which to wind a coil. The theoretical reductions in the power required by the coil itself amounted to more than 1,000, so that when the refrigerator power was lumped in, overall gains of a factor of 20 or more were still predicted. The calculations seemed so encouraging that we have since initiated a small experimental program to prove these points and are well in the way to building a small test coil. We have already uncovered a new and very simple way to purify and encapsulate sodium and have experimentally achieved a reduction in resistance of nearly 6,000 to 1 in test conductors. This represents a higher purity than the target which we set for ourselves in the design of the test coil. It appears that in a year or so we may be ready to design large coils using cryogenic principles and to predict their performance and cost.

There are three reasons why we are excited about cryogenic coils.

First, we see a chance to make some substantial savings, in a year or so, in the cost and the difficulty of generating the high magnetic fields in mirror experiments now in the planning stage. We have already realized a substantial dollar and time saving in the Alice experiment by utilizing coils cooled to liquid-air temperatures.

Second, we see a chance that this work may make a contribution to the art of generating large volumes of high magnetic field for particle accelerator and other applications.

Third, in the long-range, we see the cryogenic coil technique as one which could have a profound influence on the size, cost, and even the feasibility of fusion reactors.

I would like to explain the last point by an example. The calculated minimum size and electrical power output of any fusion reactor which uses external coils to generate its confining field is determined primarily by the power required to generate the field. The same is true of the minimum plasma pressure required to make such a reactor sustain its magnet losses. This is because the power output from the reactor depends on the volume of reacting plasma and on the amount of fuel present—that is, on the pressure of the plasma. For this reason, any substantial reduction in the magnet power required is reflected immediately in a reduction in the size of the reactor, its electrical power output, and the plasma pressure required to produce a positive power balance. We are interested in all of these factors, since most calculations of possible fusion reactors using ordinary coils indicate that they would be huge and produce uneconomically large amounts of electrical power. Furthermore, if a reactor must operate at a high plasma pressure, that is, nearer the bursting pressure of the magnetic bottle, it is much more likely to be unstable.

I will illustrate the gains which might be achievable by considering two entirely hypothetical mirror machine reactors. These mythical reactors will be assumed to utilize heavy hydrogen as the primary fuel and are required to operate at the rather low ratio of plasma pressure to the applied magnetic pressure of 10 percent, so as to minimize plasma stability problems. One reactor would be visualized as using copper coils at room temperature, the other would use sodium coils refrigerated to about 10° absolute. Note that the dimensions which are given represent the minimum size the reactor could have, consistent with producing a power balance under the assumed plasma conditions.

TABLE I.—*Hypothetical D–D Mirror machines*

	Refrigerated sodium (10° K)	Unrefrigerated copper (300° K)
Diameter of reactor chamber	10 feet	45 feet.
Length of reactor chamber	100 feet	330 feet.
Net electrical power output	50 megawatts	5,000 megawatts.
Weight of magnet	700 tons	500,000 tons (50 percent of U.S. annual copper production).

Mr. RAMEY. That is 5 million kilowatts.

Dr. POST. Yes.

Mr. RAMEY. That is a great deal of power.

Dr. POST. That is right. This is one of the things that has disturbed some people in estimating fusion power reactors.

Mr. RAMEY. It is five times the size of Grand Coulee Dam.

Dr. POST. That is right. And this is also a ridiculously large figure on the weight of the coils, although I admit I have taken a tough example, the D–D reaction. But someday the economy feasibility of fusion reactors may be wrapped up in success of the cryogenic coil technique as soon as the plasma problems are solved. So you see that any progress in reducing the power required to generate the magnetic field shows up immediately in the technology that will be required to produce a feasible reactor.

I think these figures say a great deal about the important role that cryogenics may someday play in fusion power even though we have not yet proved that we would be able to achieve the plasma conditions which are implied. May I point out however that it is possible to calculate with assurance the size and power output of a fusion reactor, even if we have not yet achieved the required plasma conditions. This is because the relevant physical quantities and other critical data such as the reaction rates and the theoretical loss rates are already well known. Although the examples I have given are for the mirror machine, any reactor which relies on the use of large volumes of externally generated magnetic fields could be expected to benefit from refrigeration of its coils.

In summary, I hope I have been able to give you a picture of our Sherwood research at the Lawrence Radiation Laboratory and the philosophy on which it is based. Furthermore, I hope it will seem as clear to you as it does to me that we have an exciting opportunity to make some real advances in the field of plasma physics. These advances cannot help but bring the goal of fusion power closer, but I feel they may do even much more than that. The example I have just given, of how the advances in cryogenic engineering—which were sparked by pure metals research and by our space and rocket programs—may someday have a great impact on the achievement of fusion power, has an obvious corollary: Work in high-temperature plasma physics has already and will no doubt continue to have an impact on our space research program—and on many other areas of our science and technology as well.

If you ask anyone at Livermore about my attitude toward our program, I am sure they will tell you that I take it very seriously. I do, and despite the difficulty of this research, I can't help myself, because I feel so strongly that the field of plasma research is of vital im-

portance to the Nation, and that no other area of present-day research has so much promise.

Representative PRICE. Thank you very much, Dr. Post.

Are there any questions? If not, thank you very much for your fine presentation. I hope you are successful in your efforts, and as successful as Dr. Christofilos has been on some of his previous efforts.

(Dr. Post's prepared statement follows:)

MAGNETIC MIRROR MACHINE PROGRAM

(By R. F. Post, University of California, Lawrence Radiation Laboratory, Livermore, Calif.)

The magnetic mirror program is centered on the investigation of heated plasmas confined in an axially symmetric magnetic field produced by cylindrical external coils. At each end of the confinement region (the mirror regions) the field is strengthened, so that the field lines come closer together. This is illustrated in the figure on page 106. The charged particles forming the plasma are then trapped in this sausage-shaped field, being reflected back and forth between the mirrors. Compression, heating, and manipulations of the trapped plasma are accomplished by controlling the current in the external coils.

The primary present goals of the magnetic mirror program are the achievement and study of confined high-temperature plasmas, since stable confinement is the key to the eventual achievement of controlled fusion power.

The effort in the mirror group is divided among five subgroups, four of which are concerned with various aspects of the physics of injection, heating, and confinement of plasmas in a mirror machine, the fifth, and newest group, is investigating a problem of technology which may be potentially very important.

A series of measurements has just recently been completed on the device called tabletop. This work represents the first and as far as we know, the only documented case of completely stable confinement of a hot plasma. There seems to be no doubt about the general validity of the result (since the plasma really stays confined—and for a long time), but we do not claim that the results are general—they may be only a special case. What is important is that this one experiment shows that it is indeed possible to confine a hot plasma stably in a magnetic field—a situation which some people were beginning to doubt could ever be reached.

In the tabletop experiments the heated and confined plasma is produced by injecting a small burst of low-temperature plasma into the evacuated confinement region while the magnetic field is very low, and then increasing the field slowly so as to compress and heat the plasma. Several years ago we found that it was possible to heat and hold a plasma confined by this process, and reported these results at the U.N. 1958, Geneva Atoms-for-Peace Conference. However, to show that a plasma is absolutely stably confined one must do more than simply demonstrate some confinement. One must show not only that the plasma is held for a time very much longer than the theoretical time in which instabilities should develop, but also that the particles escape across the magnetic field and out of the bottle at a rate which is consistent with the very slow theoretical leakage rate that any magnetic bottle should exhibit. Furthermore, the significance of the experiment is greater if the confined particles represent an "interesting" plasma; that is, one in which the particle density is comparable to that required in a fusion reactor. Also, at least one of the charged-particle components of the plasma (electrons or ions) should have a temperature in the thermonuclear range, and the plasma should have dimensions which are at least several particle-orbit-diameters thick, so that it looks like a confined gas, and not like a group of particles moving in a highly organized way.

Since Geneva the measurements on tabletop have resulted in much more accurate determinations of the density and temperature of the plasma, and of the rate of escape of the particles. The particle densities have been found to be about those which would be required for a self-sustaining reactor, and the electron temperature has been measured at some 400 million degrees, which is well into the thermonuclear range. Because of the injection method used, the ions did not get as hot, certainly not over some 10 million degrees. It must be emphasized, however, that since we were not looking for thermonuclear reactions, but for a provable case of stable confinement, the fact that the electrons rather than the

ions possess the highest temperature is not particularly important, just as one could in principle demonstrate the aerodynamic principles of aircraft flight in a wind tunnel just as well by using carbon dioxide as by using air. The result would be a special case, but everyone would still believe that flight was possible.

What has been found in these studies is that, within the accuracy of the measurements, not only is the plasma confined some 100,000 times longer than the theoretical instability times, but it appears to be leaking out only slowly, and at a rate which agrees with the theoretical minimum rates for a perfectly stable plasma. What is most encouraging is the discovery that the rate of leakage across the field lines (as opposed to leakage out the ends) is almost immeasurably small, again in agreement with the theory for a stable plasma. This is illustrated in the figure on page 109, which shows the density distribution of the compressed plasma at various times after the initial compression. As can be seen from the curves, the plasma diffuses only a small amount, if at all, during the confinement. The measurements show that this rate of diffusion must be not greater than about 1 foot per second, which is about one-billionth of the velocity of the confined electrons themselves. Thus in the course of diffusing out 1 inch across the magnetic field in Table Top, each electron in the plasma will move back and forth in the chamber over a total distance about equal to the diameter of the earth.

Since the publication of these results, new data have been obtained that may be scientifically of almost equal importance. It has been found possible to compress the plasma so greatly and crowd the particles so closely in the middle of the machine that the plasma becomes unstable and begins to diffuse rapidly across the field. The existence of this instability was predicted theoretically before it was produced experimentally. It is possible to induce or eliminate this instability at will, by controlling the plasma conditions. These new experiments provide a powerful new experimental tool by which to investigate the laws of stability of a plasma, since one can venture into instability land as far as he likes, but can always come back and touch base when he feels the need.

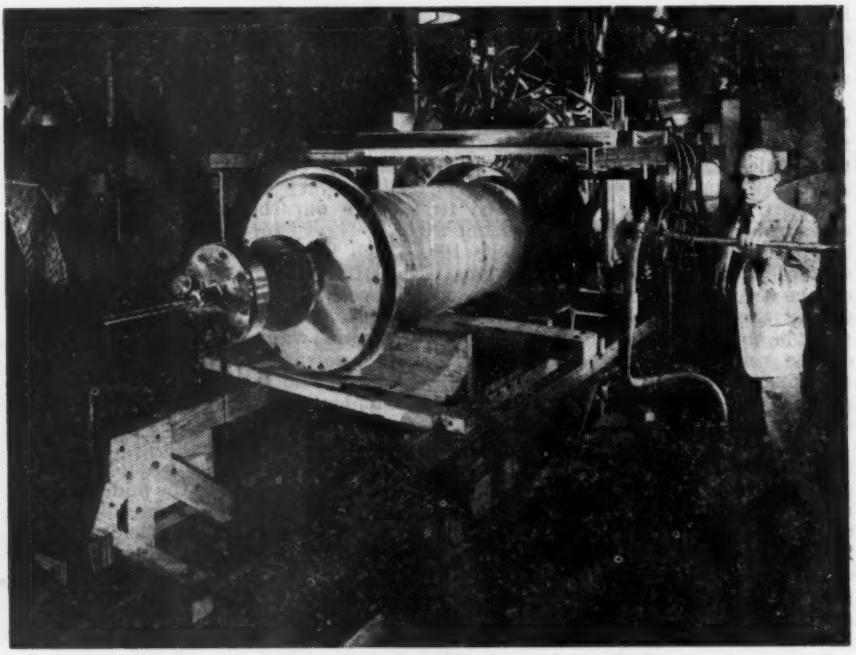
Encouraging as these various experiments are, it is just as important to be aware of what they do not demonstrate as to understand what they do prove. They are not thermonuclear experiments, since the ions would have to be several times hotter to produce thermonuclear reactions; they are carried out with a volume of plasma much smaller than that required for a thermonuclear reactor; they still are subject to appreciable experimental inaccuracies; and they are conducted under conditions where the electrons are hotter than the ions, a fact which might be important for subtle reasons of which we are not now aware.

Work is therefore proceeding in other parts of the mirror program to remove these restrictions, and to explore plasma stability under more general circumstances where the ions should be much hotter than the electrons and should possess temperatures in the thermonuclear range. Among these experiments are the following:

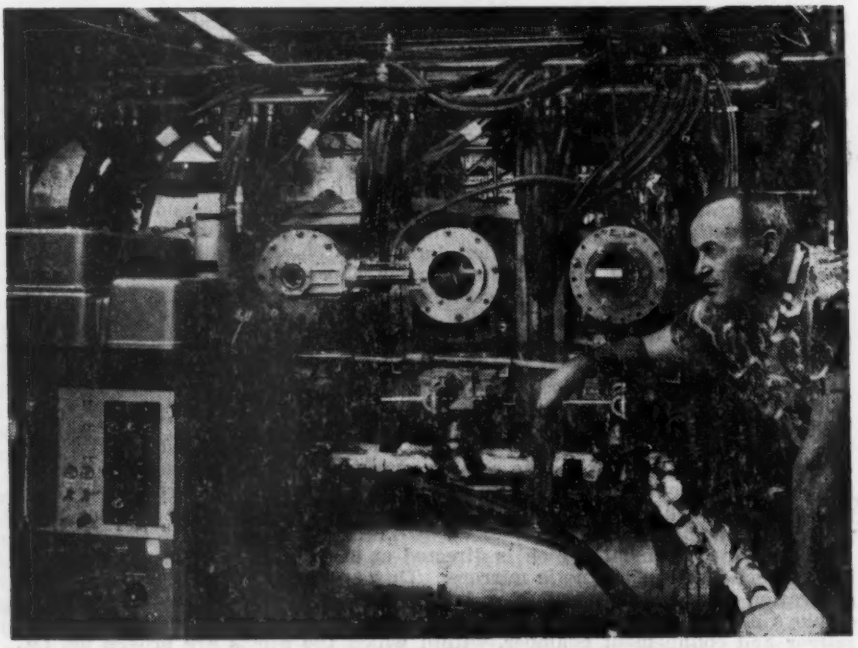
The Toy Top experiment, which centers around the use of successive or multistage compression of a plasma. This is accomplished by using pulsed magnetic mirrors, together with a programed transfer of the plasma along the machine into regions of smaller and smaller diameter. This procedure results in the possibility of reaching much higher plasma energies by compression as compared with a single-stage magnetic compression.

The general principles of operation of Toy Top have already been demonstrated, but only with plasmas of low ion temperature. Recent studies have been devoted to improvement of the plasma sources, to the point where quite high ion energies are achieved, and to the study of trapping of these plasma beams into the first section of the machine. The equipment used in these plasma trapping experiments is shown in top figure, page 116. The results of these studies have been incorporated into a design for an improved machine, now under construction. During the coming year operation of the new machine is planned, which could in principle result in the achievement of a relatively dense plasma of much higher ion temperature than heretofore possible, confined for a period of some milliseconds.

The Alice experiment, which is directed at a different method of creating a hot plasma between magnetic mirrors. This approach, which may permit the generation and maintenance of a hot plasma under steady-state conditions, employs an intense beam of energetic neutral atoms. This beam crosses the mirror and confinement chamber, within which the atoms are broken up, i.e.,



High energy plasma injection experiment (F. Coensgen).



P–4 high ionization plasma generator.

ionized by collisions with other charged particles. This breakup results in trapping a fraction of the injected particles and thus provides an injection mechanism, i.e., a means for continuously replenishing the confined plasma. Theoretical studies of the Alice experiment were made, and many of the components have been constructed. A small model of Alice, called Baby Alice, has already been constructed and is in operation. This will be used to investigate some of the problems of the initial buildup phase. This is shown in the figure on page 110.

One of the major problems in the Alice experiment is to achieve very good vacua in the confinement chamber.

Final assembly of Alice is underway. This will be followed by intensive study of the injection and buildup phenomena.

The P–4 facility, which is an operating device which produces a relatively dense, highly ionized, but low temperature (5 ev), plasma column in steady state. It is being used to study various properties of a plasma which are of both general interest to the Sherwood program, and of specific interest to some of the impurity, instability, and injection problems in the mirror machine program. P–4 is shown in photograph on page 116.

In the fifth component of the mirror program we are making a serious attempt to bring a very old idea to practical reality. The idea is that it should be possible to greatly reduce the huge powers needed to generate large volumes of intense magnetic fields by the following means: It is well known that the electrical resistance of a metallic conductor becomes less if it is cooled to a low temperature (I am not here referring to the phenomenon of superconductivity, which cannot exist in the presence of a high magnetic field). Reducing the electrical resistance of the conductors in a magnet coil will of course reduce the power required to produce a given magnetic field. However, we can only hope to save in the overall energy cost of generation of a high magnetic field if the power required to energize a refrigerated coil plus that required to run the refrigeration plant itself is less than that to run an ordinary coil at room temperature. Now at very low temperatures, say 20° above absolute zero, the usual factor which limits the drop in electrical resistance which can be attained by cooling is simply the purity of the conductor. This must be very good to achieve a low resistance. Thus if we had set out as recently as 10 years ago, to calculate the improvement achievable by refrigeration with the data then available we would have no doubt been led to the pessimistic conclusion that little if any gain would be possible.

However, the last decade has seen a revolution in the technology of superpure metals, in the physics of electrical conductivity at low temperatures, and particularly in the technology, the size and the efficiency of cryogenic equipment (that is, ultra-low temperature refrigerators). We can trace this last fact directly to the development of cryogenic engineering techniques stimulated by the missile and space programs.

Therefore about a year and a half ago, in the course of studying the minimum size of a mirror machine, when the question of refrigerated coils was again examined, it was discovered that the theoretical calculations indicated that a large reduction in both power requirements and in dollar cost of high magnetic fields could be expected. It appeared that either very high purity aluminum, or high purity sodium encapsulated in stainless-steel tubes, should make very good conductors from which to wind a coil. The theoretical reductions in the power required by the coil itself amounted to more than 1,000, so that when the refrigerator power was lumped in, overall gains of a factor of 20 or more were still predicted. We have since initiated a small experimental program to prove these points and are well on the way to building a test coil. We have already uncovered a new and simple way to purify and encapsulate sodium and have achieved a reduction in resistance of nearly 6000:1 in test conductors. This represents a higher purity than the target which we set for ourselves in the design of the test coil. It appears that in a year or so time, we may be ready to design large coils using cryogenic principles and to predict their performance and cost.

There are three reasons why we are excited about cryogenic coils. First, we see a chance to make some substantial savings, in a year or so, in the cost and the difficulty of generating the high magnetic fields in mirror experiments now in the planning stage. We have already realized a substantial savings in dollars and in time in the Alice experiment by utilizing coils cooled to liquid air temperatures. Second, we see a chance that this work may make a contribution to the art of generating large volumes of high magnetic field for particle accelerator and other applications. Third, in the long-range, we see the cryogenic coil technique

56108 O—60——8

as one which could have a profound influence on the size, cost and even the feasibility of fusion reactors.

The last point can be explained by an example. The calculated minimum size and electrical power output of any fusion reactor which uses external coils to generate its confining field is determined primarily by the power required to generate the field. The same is true of the minimum plasma pressure required to make such a reactor sustain its magnet losses. This is because the power output from the reactor depends on the volume of reacting plasma and on the amount of fuel present—that is, on the pressure of the plasma. For this reason, any substantial reduction in the magnet power required is reflected immediately in a reduction in the size of the reactor, its electrical power output, and the plasma pressure required to produce a positive power balance. We are interested in all of these factors, since most calculations of possible fusion reactors, using ordinary coils indicate that they would be huge and produce uneconomically large amounts of electrical power. Furthermore, if a reactor must operate at a high plasma pressure, that is, nearer the bursting pressure of the magnetic bottle, it is much more likely to be unstable.

The gains which might be achievable can be illustrated by considering two entirely hypothetical mirror machine reactors. These mythical reactors are assumed to utilize heavy hydrogen as the primary fuel and are required to operate at the rather low ratio of plasma pressure to the applied magnetic pressure of 10 percent, so as to minimize plasma stability problems. One reactor would be visualized as using copper coils at room temperature, the other would use sodium coils refrigerated to about 10° absolute. Note that the dimensions given represent the minimum size the reactor could have, consistent with producing a power balance under the assumed plasma conditions.

TABLE I.—*Hypothetical D–D Mirror machines*

	Refrigerated sodium (10° K)	Refrigerated copper (300° K)
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Net electrical power output	50 megawatts	5,000 megawatts.
Weight of magnet	700 tons	500,000 tons (50 percent of U.S. annual copper production).

These figures seem to say a great deal about the important role that cryogenics may someday play in fusion power, even though we now have no proof that we would be able to achieve the plasma conditions which are implied. It should be noted, however, that it is possible to calculate with assurance the size and power output of a fusion reactor even if we have not yet achieved the plasma conditions necessary to produce the reaction. This is because the relevant critical data such as the reaction rate and the theoretical loss rates are already well known. Although the examples given are for the Mirror machine, any reactor which relies on the use of large volumes of externally generated magnetic fields could be expected to benefit from refrigeration of its coils.

(Two additional papers on thermonuclear research follow:)

PLASMA RESEARCH

W. R. Baker and W. B. Kunkel
University of California, Lawrence Radiation Laboratory
Berkeley, California

The Berkeley program is characterized by diversity and by considerable emphasis on fundamental plasma research. By virtue of its location the site is easily accessible to students at the University of California and is also open to foreign scientists for extended visits. At the present time six graduate students (five experimentalists and one theorist) are working towards Ph.D. degrees in the field of plasma physics. They are engaged in investigations of hydromagnetic waves, diffusion across magnetic fields, electron beam interaction and ionization rates in crossed electric and magnetic fields. In view of the growing demand for scientists trained in this field, this activity at Berkeley is considered to be of the greatest importance and it is planned to maintain this level of student participation, or perhaps even gradually increase it in the foreseeable future.

The Berkeley site currently is also host to five foreign visitors (two Australian, two French and one British) who are sent for the purpose of gaining experience in the field of high temperature plasma and controlled fusion research. It is expected that this activity, too, will continue, consistent with the policy of the AEC favoring exchange of personnel with foreign laboratories in this field.

The following project description will include the major experiments only:

The Tubular Pinch

It is predicted theoretically that a flat current-carrying plasma sheet of infinite extent is not subject to the well-known instabilities observed in the ordinary cylindrical or toroidal pinch discharges. This idea is utilized in an approximate fashion in the Tubular Pinch device (Triax) where an edgeless sheet plasma is formed in the shape of a straight cylindrical sleeve by passing a high current through deuterium at low gas density along the annular space between two coaxial cylindrical return conductors (see

Fig. 1). The stability of the Tubular Pinch has been experimentally confirmed under low-power conditions where the temperature is below one million degrees. At high levels the behavior becomes complicated, with neutron emission and large power losses notable. The neutron energies were examined, giving some evidence against hydromagnetic instability. Not all the subsequent experiments were aimed directly at the high-level problems; somewhat more basic studies of the pinch formation, heating, and containment seemed necessary as well. These have utilized, among others, the techniques of spectroscopy, microwave transmission, magnetic probes and Langmuir probes. Recently a streak camera has been available. It has shown vividly that the plasma boundaries may remain cylindrical for long periods. It has also exposed some of the reasons for the high-power difficulties. Solutions for these problems are being found, and the production of stable plasmas in the 5 million degree range seems likely in the not too distant future.

The Rotating Plasma Experiments

The confinement of ionized gas by magnetic mirrors is expected to be greatly enhanced if the plasma as a whole is spinning about the axis of symmetry at speeds larger than the mean thermal speed of the ions. In the rotating plasma device (Homopolar) the gas is ionized and set into rotation by transiently passing a large radial current between coaxial electrodes in the presence of an essentially axial magnetic field. In the Berkeley experiments two configurations have been tried so far (see Fig. 2): (a) the flat disc geometry (Homopolar I and II) and (b) the curved field geometry (Homopolar III). To date it has not been possible to produce rotational speeds in excess of a few centimeters per microsecond. Present and future investigations are aimed at finding the reason and possibly the cure for this limitation. Shunting currents flowing along insulating surfaces are feared to be associated with the phenomenon. Such currents have been observed in Homopolar III in spite of the special design intended to prevent their appearance. In Homopolar IV, which is under construction, neutral gas will be injected radially into high vacuum as a

brief pulse (of the order of 10 μsec duration) in the center of a very long cylindrical tube. Axial magnetic and radial electric fields are established prior to injection. The gas will break down and start rotating before it can expand very far axially so that the insulators which are located at the ends are never exposed to plasma. If the velocity limit is still observed in the absence of shunting currents a search for unsuspected instabilities will have to be carried out. If the velocity limit can be raised methods of heating the plasma will be investigated.

Hydromagnetic Waves and Ion Cyclotron Heating Experiments

In these experiments highly ionized but not necessarily very high temperature plasma is created by some discharge in a cylindrical geometry containing an axial magnetic field. The first goal, already largely accomplished, was to verify experimentally in some detail the existence and properties of waves predicted by simple hydromagnetic theory. This was accomplished by applying a time-varying radial electric field at one end of the cylindrical plasma (see Fig. 3), thus launching torsion waves in the fluid, and observing the propagation along the magnetic field of various detectable disturbances. Fundamental wave properties such as velocity, attenuation and induced magnetic fields have been measured. This work is continuing with several refinements. In a second experiment plasma heating by this means is being investigated. A hydromagnetic wave is induced at one end of a magnetic mirror geometry such that ion cyclotron resonance occurs in the center and the wave energy is transferred directly to the ions. This effect is being investigated by microwave interferometer measurements of electron density, monochrometer observations of emission spectra, reaction of the plasma on external circuit parameters, probe measurements of induced fields and by neutron detection.

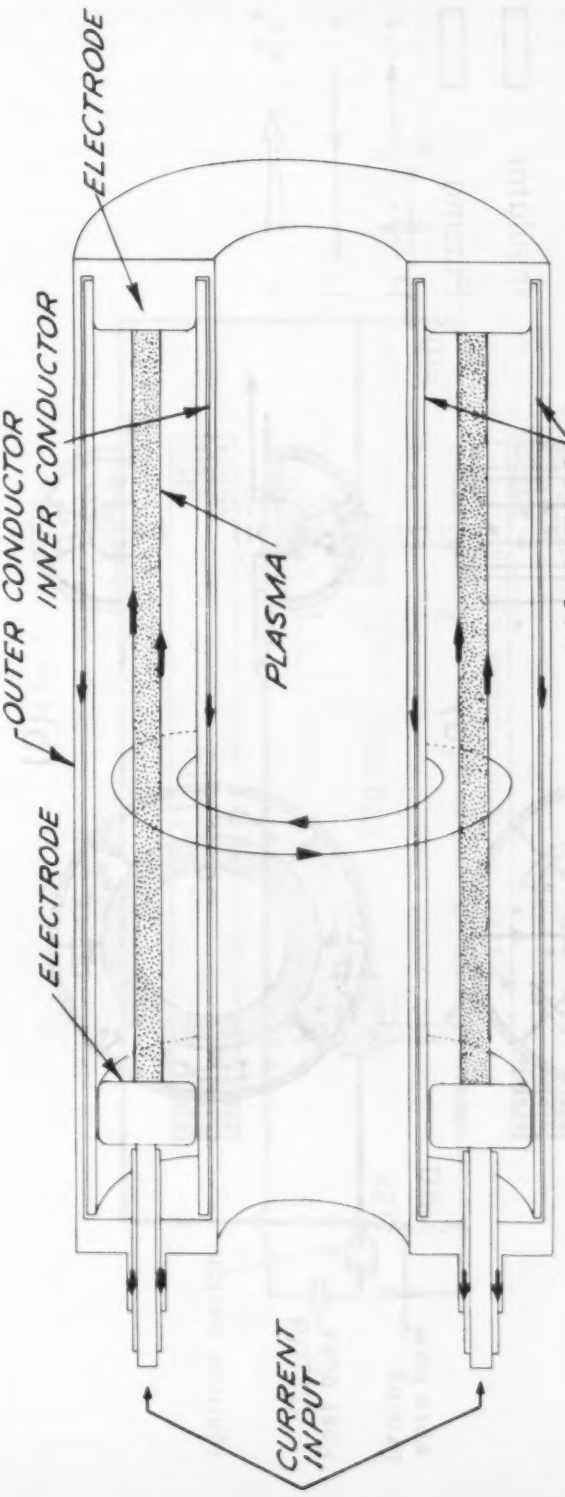
Plasma Waves and Ion Diffusion

Several experiments on cool and/or weakly ionized gases are in progress for the purpose of furthering knowledge of charged particle diffusion (including enhanced diffusion), beam-plasma interactions, and the propagation

56108 O—60——9

of electrostatic and electromagnetic waves in bounded systems. Two experiments concerning the enhancement of charged particle losses from a magnetically confined positive column, and localized plasma-density measurements by means of microwaves guided by dielectric coated wires are nearing completion. The construction of a thermally-ionized cesium plasma column is nearly finished.

Studies of the convective and non-convective instabilities associated with the traversal of plasmas by charged particle beams will continue. The diffusion and conductivity of the cool, highly-ionized cesium plasma will be studied, and large-amplitude, low-phase velocity electrostatic waves will be propagated on this plasma column for the purpose of studying enhanced diffusion, particle acceleration, and non-linear wave properties. Methods of measuring the temperature, density, and other physical properties of plasmas will be examined further.



TRIAX PINCH DEVICE

Fig. 1

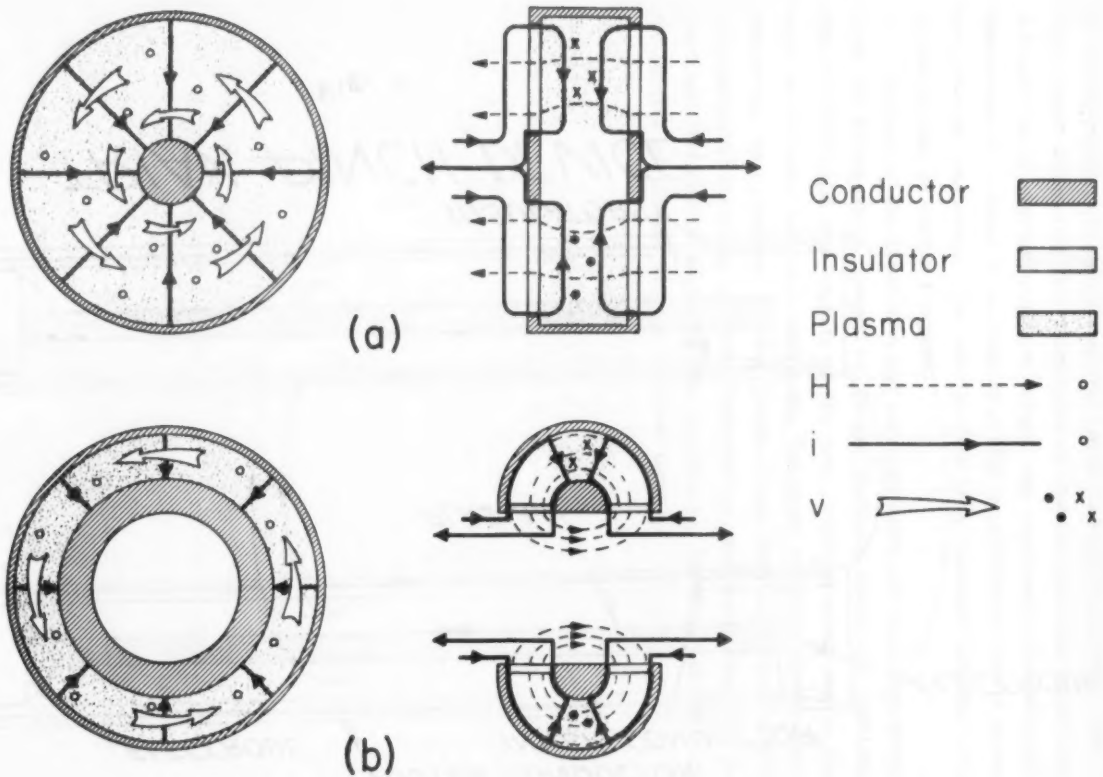


Fig. 2
Alternative forms of Homopolar Device.

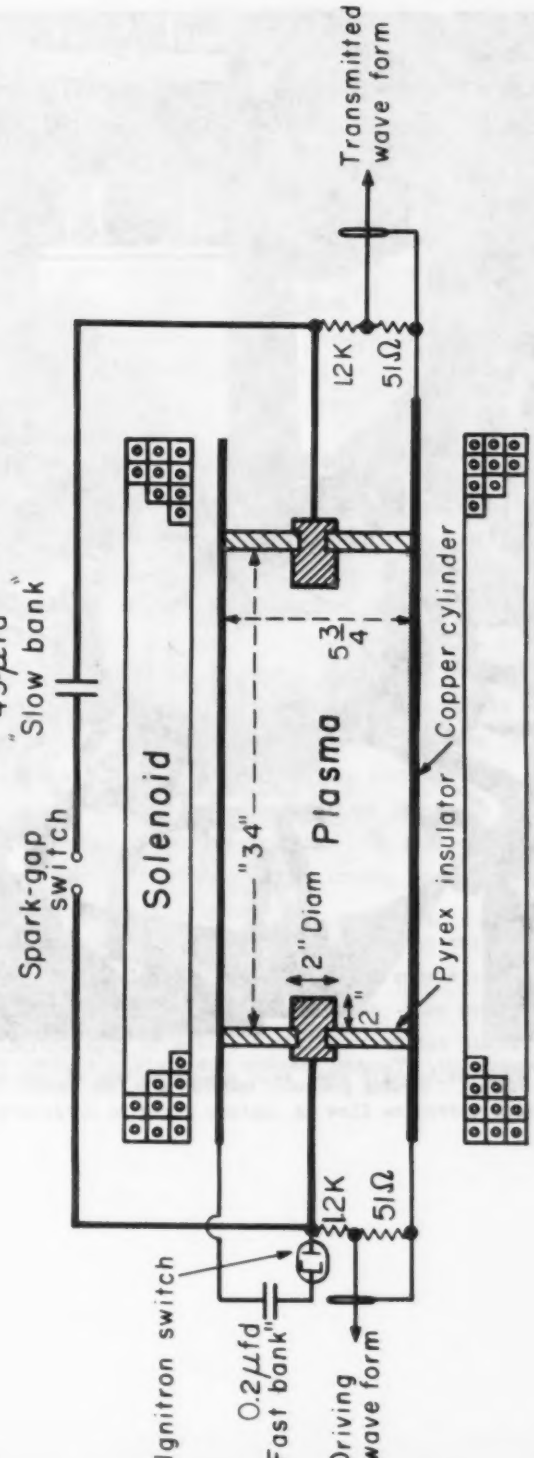


Fig. 3

Hydromagnetic Wave Experiment.

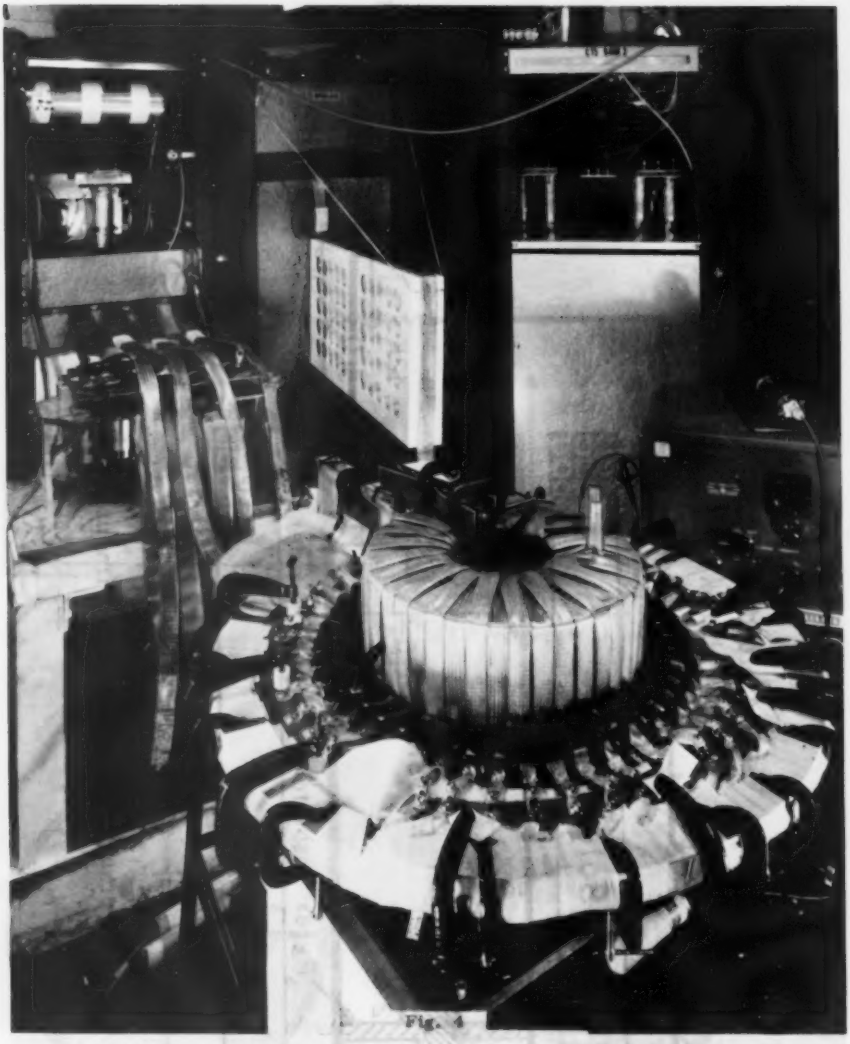


Fig. 4

Small levitron experiment. Toroidal chamber with 5/8 in. o.d. center ring is inside braid winding. Winding generates fast-rising B_z for pinch.

PINCH RESEARCH

S. A. Colgate and H. P. Furth
University of California, Lawrence Radiation Laboratory
Livermore, California

At the beginning of this decade, the word "pinch" referred to a simple self-constricted gas discharge between two electrodes. Mostly as a result of activity in controlled thermonuclear research, the basic pinch idea has been enlarged, and an up-to-date definition of it must be phrased in more abstract terms: pinch research is the study of plasma confinement in closed magnetic field structures that depend significantly on currents flowing in the plasma itself. By a closed structure is meant one from which plasma cannot leak directly, but only by gradual diffusion across magnetic field.

Every "magnetic bottle" necessarily makes use of some plasma current, and therefore has to some extent the aspect of a pinch. The distinguishing feature of pinch research is its emphasis on configurations where the plasma current is comparable in magnitude to currents in external coils and conductors. This study is attractive from the practical point of view, because large plasma currents in closed structures go hand in hand with large plasma densities and pressures, with short burn-up times of thermonuclear fuel, with small fusion reactor sizes, and with sound prospects of economic viability. At the same time, pinch research is attractive to the plasma physicist, because of the relative simplicity of the experimental apparatus needed to test and enlarge plasma theory in non-trivial ways. For these basic reasons, pinch research has been a major approach to the control of fusion power from the very beginning, in all the countries that have undertaken experimental programs.

From the point of view of pinch research at the Lawrence Radiation Laboratory, the results to date have proved consistent with the favorable initial prospects. There has been a step-by-step evolution of pinch design towards increasingly effective containment of hot plasma; and at each step partly or wholly new phenomena have been discovered that have stimulated further evolution of pinch design, as well as better understanding of plasma

physics in general. A schematic view of progress in pinch research at the Berkeley and Livermore sites is given in Table I, with some footnotes to indicate original contributions by the LRL research groups.

Our philosophy of operation has been to pursue the physics of large currents in plasma, rather than to concentrate on the advancement of any highly technical plan toward the reactor stage. This philosophy has had the combined advantage of facilitating original experimental approaches, and of avoiding heavy commitments to engineering schemes, which in the course of a rapidly evolving physics understanding often turn out to be impractical and yet resist modification because of the large investments involved.

Emphasis on basic physics research has the inherent virtue that progress tends to be cumulative rather than oscillatory. Between the present statement of our research position and the statement submitted to this Committee two years ago, there is a close consistency of outlook. Accordingly, the conclusion of the earlier testimony may serve as introduction to a more detailed discussion of our present work. "We feel very strongly that, for the practical purpose of developing a fusion reactor out of the stabilized pinch, neutronics are not at present of demonstrable value, and the emphasis should be placed on general plasma physics. In particular, a great effort must be made to clean up such obvious faults as low purity, low electron temperature, and poor containment. If this is done successfully, then neutron production of high magnitude will automatically result, and furthermore there may be a chance of authenticating thermonuclear origin."

During the first six months of 1958, the Livermore Research Group studied in some detail the properties of a high-powered toroidal "stabilized pinch". We arrived at a number of conclusions that were radical at the time, but that have since become widely accepted.

1. The observed neutron production was of non-thermonuclear origin, and was therefore not indicative of high ion temperature.

2. The conductivity of the plasma was very low, and so was the electron temperature.

3. Most of the input energy into the plasma was lost immediately to the tube wall.

4. Ultraviolet impurity radiation was responsible for a large fraction of this energy loss, but not for all of it.

5. Reduction of the plasma impurity content could not be expected to prove a panacea of the other troubles, unless the observed hydromagnetic turbulence of the "stabilized pinch" were also suppressed.

These conclusions were reported in part at the Washington Sherwood Conference of February 1958, and more completely at the Second Conference on the Peaceful Uses of Atomic Energy, at Geneva in August 1958. Thereafter our toroidal "stabilized pinch" experiment was discontinued in favor of more rudimentary experiments to determine the origin of the hydromagnetic turbulence, and to suppress it, if possible. These experiments have taken three separate courses:

1. The modification of the "stabilized pinch" design to demonstrate the dependence of stability on magnetic field geometry.

2. The replacement of plasma by fluid metal, to isolate the purely hydromagnetic aspect of pinch behavior.

3. The direct exploration of turbulence by passage of a high-energy electron beam "probe" through the pinch.

While all of these approaches have yielded useful information, the first has proved most constructive, since it affords both a clue to the basic problem and also the means of its solution.

A highly flexible linear pinch apparatus (Fig. 1) was constructed, containing a metal center-rod (like the tubular or "Triax" pinch tubes of Anderson and Baker), as well as an external coil for the generation of longitudinal magnetic field. With this apparatus it was possible to approximate the various pinch configurations that had been tried previously, as well as a larger class that had not been studied before, and to which we have given the name "hard-core pinches". In the course of this experimentation, the hydromagnetic stability theory was substantiated over a wide range of predictions, but at high power levels and low plasma densities its

stability conditions were generally too weak. Thus, the conventional "stabilized pinch" (Fig. 2a), where the margin of theoretical stability is small, was always highly turbulent, while its geometrical inverse (Fig. 2b), where the margin of theoretical stability is large, was only slightly unstable at the same power levels. Minor modifications of the "inverse stabilized pinch" configuration, such as the provision of some longitudinal magnetic field throughout the tube, have led to discharges that were perfectly stable within our ability to ascertain, and within the range of power input and plasma density that the apparatus permitted.

A linear pinch device with longitudinal magnetic field is limited to relatively low plasma temperature because of thermal conduction to the electrodes. To determine whether the observed suppression of hydromagnetic turbulence in hard-core pinches permits improved plasma conductivity and plasma energy containment, we have therefore gone on to the toroidal geometry (Fig. 3). In that case, the center-rod becomes a center-ring, which is held levitated by the same magnetic field structure that confines the plasma. We call this device a "levitron".

A high-powered levitron of 12" bore, designed for high-purity and, hopefully, high-temperature operation, will reach completion in the next few months. The levitation process and the basic pinch-cycle have already been tested on a small low-powered model (Fig. 4). In the small levitron, as in the linear experiments, the "inverse stabilized pinch" has been found much more stable than the conventional "stabilized pinch".

The largest effort on hard-core pinches is currently at Livermore. A modestly supported program (with no toroidal experiment) is under way at Harwell, England. The Soviets have not disclosed the existence of a hard-core pinch program, but have mentioned the basic idea in theoretical papers.

Our present estimate of the status and prospects of pinch research is the following:

1. The stability requirements on the pinch are more restrictive than had been expected, presumably because of the disturbing influence of the pinch-current flow in the plasma. Accordingly, the prospects of the conventional "stabilized pinch" appear doubtful.

2. Hydromagnetic turbulence due to pinch-current flow can be suppressed in hard-core pinches. If no new forms of instability arise, a variety of these configurations is expected to have adequate stability for the confinement of a hot plasma under conditions of practical interest.

3. If new forms of instability do arise, they will presumably be of the electrostatic sort, and will be caused by the component of the pinch current that flows along magnetic field. Such instabilities can be avoided if we restrict ourselves to a particular class of hard-core pinches in which the current flows only across magnetic field ("maximal-force pinches").

4. The stable containment of hot plasma in the levitron is thus consistent with a reasonable, and even with a pessimistic, interpretation of present-day plasma theory.

The levitron experiment is seen by us primarily as a test of the theories of plasma stability and containment. The basic design can be extrapolated to a fusion reactor, but a detailed presentation of this kind would be premature. A more fundamental association between the levitron experiment and the long-range aims of the controlled thermonuclear research effort can be made in the following terms: the pinch regime is a priori the most favorable regime for a fusion reactor, because of its characteristics of high plasma current and complete plasma enclosure; a consistent experimental study is certainly warranted to determine to what extent actual fusion reactors will be able to approximate the ideal pinch regime, and what will be the physical limitations involved.

Table I. Progress in Pinch Research

Configuration	Initiation of Research	Objective Achieved	Generation of Neutrons	Shortcomings of Experimental Configurations	Diagnosis of Neutron Production
Simple Dynamic Pinch	1952	High-pressure plasma generated and confined	1955[1,3]	Gross hydromagnetic instability	Caused by gross instabilities[2,3]
"Stabilized" Pinch[4,6]	1956	In addition to the above, suppression of gross hydromagnetic instabilities	1957	Small-scale hydromagnetic turbulence	Caused by small-scale turbulence[5,6]
Tubular or "Triax" Pinch[7,8]	1956	" " " "	1957[7,8]	Small-scale hydromagnetic turbulence	Cause not identified
Hard-core Pinch[9,10]	1958	In addition to the above, suppression of small-scale hydromagnetic turbulence	Not attempted	Possible presence of electrostatic instabilities	- - -
Maximal-force Hard-core Pinch[10]	1960	In addition to the above, predict suppression of electrostatic instabilities, if any	--	Experiment not yet carried out	- - -

Original Contributions of LRL Pinch Program

1. W. R. Baker and O. A. Anderson, Pinch Techniques and Measurements, UCRL-3207, (Nov. 1955).
2. S. A. Colgate, Neutron Production in Pinch due to Instability Breakup, UCRL-4702, (July 1956).
3. Anderson, Baker, Colgate, Ise, and Pyle, Phys. Rev. 110, 1375, (1958).
4. S. A. Colgate, The Stabilized Pinch, UCRL-5036 (July 1956).
5. Colgate, Ferguson, and Furth, Non-Thermonuclear Neutrons from the Stabilized Pinch, UCRL-5118, (Jan. 1958).
6. Colgate, Ferguson, and Furth, 2nd U.N. Conference on Peaceful Uses of Atomic Energy, 32, 129 (1958).
7. Anderson, Baker, Ise, Kunkel, Pyle, and Stone, The Triaxial Pinch Device, UCRL-5862, (Feb. 1960).
8. Anderson, et al, 2nd U.N. Conference on Peaceful Uses of Atomic Energy, 32, 150 (1958).
9. S. A. Colgate and H. P. Furth, Small-Scale Instabilities of the Pinch and a Suggested Remedy, UCRL-5392, (Nov. 1958).
10. S. A. Colgate and H. P. Furth, Stabilization of Pinch Discharges, UCRL-5862 (Feb. 1960).

10. S. A. Colgate and H. P. Furth, Stabilization of Pinch Discharges, LAMS-3002 (Dec. 1963).

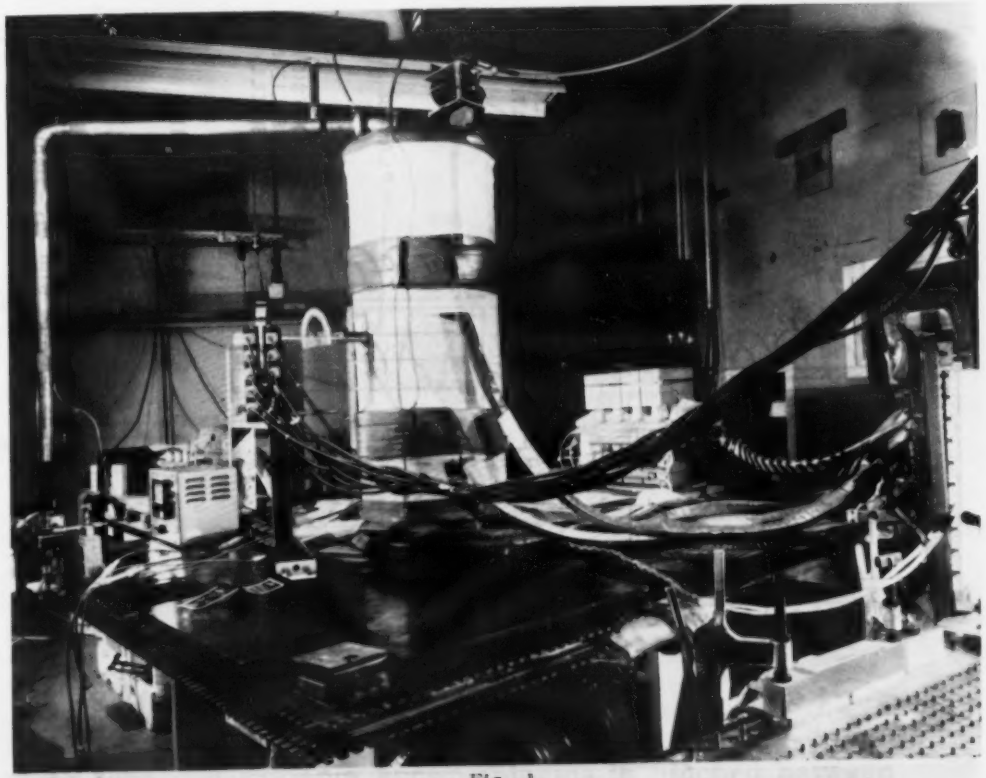


Fig. 1

Linear Hard-core Pinch Experiment. 12" i.d. quartz tube with 3" o.d. center rod inside the large coil.

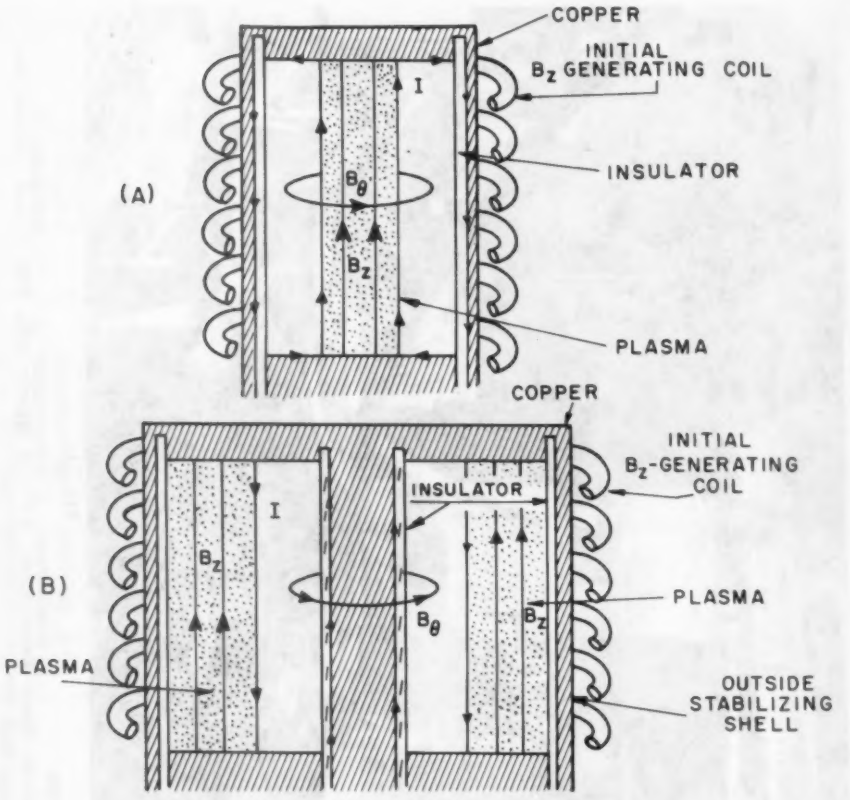


Fig. 2

A. Conventional "stabilized pinch".
B. "Inverse stabilized pinch" in hard-core pinch tube. Configuration can be assembled either by pinching outward with B_θ - field as shown, or by pinching inward with B_z, as is done in toroidal analogue.

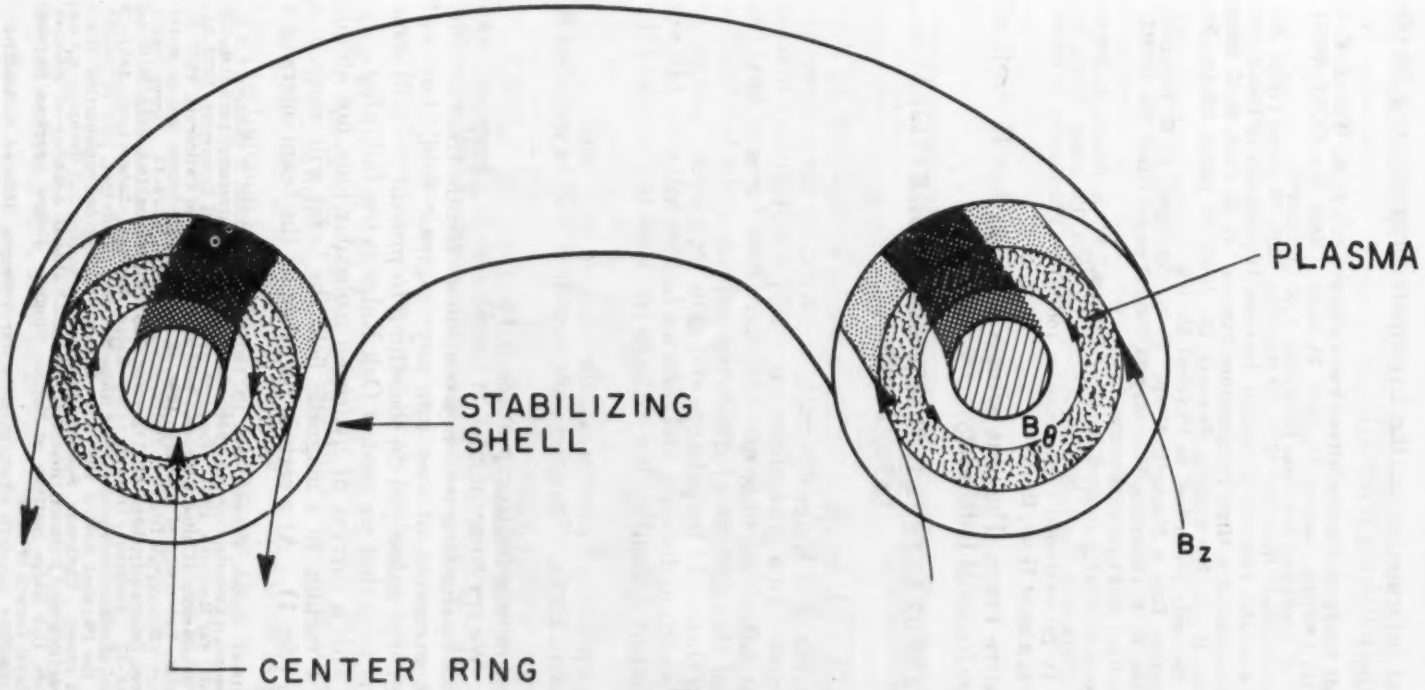


Fig. 3

Toroidal hard-core pinch tube. In example shown, plasma is pinched between internal B_Θ - field and external B_z - field, thus making an "inverse stabilized pinch".

(Additional information on the Livermore program may be obtained from the following reports:)

"Experimental Study of Plasma Alfven-wave Properties," J. M. Wilcox, F. I. Boley and A. W. DeSilva, August 27, 1959, 21 pages (Rept. No. UCRL–8885). Available Office of Technical Services, Department of Commerce.

"Astron Thermonuclear Reactor," N. C. Christofilos, 1958, 28 pages (Rept. No. UCRL–5032). Available Office of Technical Services, Department of Commerce.

"Stable Confinement of a High Temperature Plasma," R. F. Post, R. E. Ellis, F. C. Ford and M. N. Rosenbluth, January 28, 1960, 13 pages (Rept. No. UCRL–5788–T Revised) Submitted to Physical Review.

"Plasma Injection Into a Magnetic Cusp," F. H. Coensgen, A. E. Sherman, W. E. Nexsen and W. F. Cummins, February 24, 1960, 7 pages (Rept. No. UCRL–5894–T). Submitted to Physical Review.

"Viscous Effects in Highly Ionized Rotating Plasma," W. R. Baker, A. Bratenahl, A. W. DeSilva and W. B. Kunkel, August 11, 1959, 13 pages (Rept. No. UCRL–8861). IV International Conference on Ionization Phenomena in Gases, Uppsala, Sweden, August 17–21, 1959.

Representative PRICE. The next witness is Dr. Arthur H. Snell, of the Oak Ridge National Laboratory. Dr. Snell.

STATEMENT OF ARTHUR H. SNELL,[1] OAK RIDGE NATIONAL LABORATORY

Dr. SNELL. I wish to speak to the question of how far we have progressed in the Oak Ridge program, and what are the prospects as they appear now. In a generalized summary, one might say that we have run into difficulties, some of which have been foreseen, but that we understand this first set of difficulties sufficiently to feel that we can overcome them. If by patience and skill we can do so, then we will proceed a certain distance, and then we feel we will probably run into another set of difficulties, but we have yet to see the nature of the second set.

To enlarge upon this I have to explain what we are doing.

Representative PRICE. Your complete statement will be included in the record.

(The statement referred to appears on p. 143.)

Dr. SNELL. We try to accumulate a hot plasma in a magnetic containing volume by shooting it in in a continuous stream from outside. If you shoot a current of ions into any magnetic field, they will promptly reemerge unless you do something to prevent it—pull some trick—and the trick that we used at Oak Ridge is the following:

You start with a current of hydrogen molecular ions, not atomic ions, and in a vacuum in a magnetic field the beam will move in a circular path (fig. 1). At a certain point we let the beam intersect a

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Research contributions in stark effect in molecular hydrogen, induced radioactivity, the nuclear isomers of Bromine, identification and assignment of delayed neutron activities, radioactive decay of the neutron, neutrino recoil spectrometry, beta recoil spectrometry, charge spectrometry.

high current vacuum arc. When that happens, there are three principal consequences. Some of the ions go through. Others are dissociated. A neutral fragment will not be contained in the magnetic field, but will go right on out. But the charged fragment of the molecular ion will travel in a circle half the size of the original circle. So if you have a continuous current of molecular ions coming in, the atomic ions will build up, or accumulate, continuously circulating in the smaller circle. The idea, then, is to keep them as long as possible in this piled up condition.

When one goes a little closer to reality, looking at figure 2, we see the magnetic coils that provide the containing field, the vacuum arc going longitudinally through them and the vacuum tank. The molecular ion beam is accelerated to 600 kilovolts outside, which is our

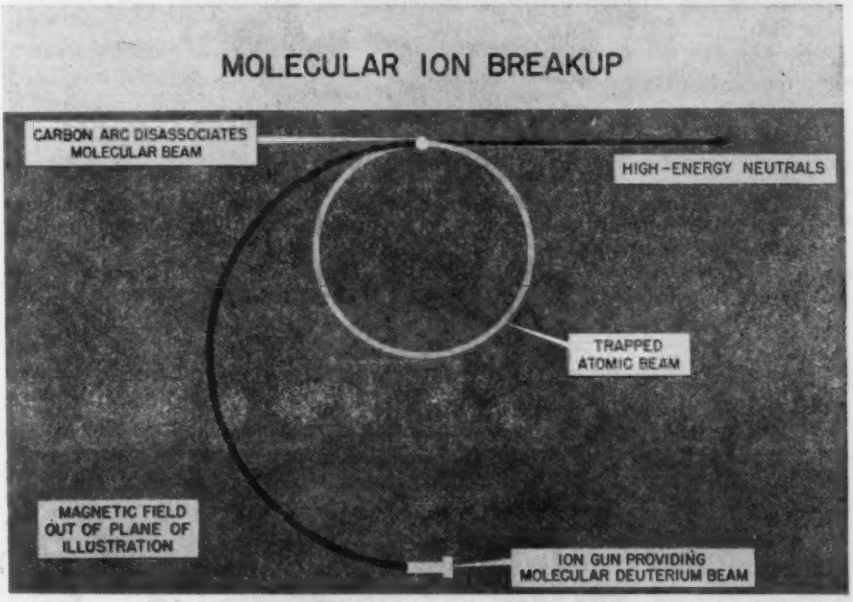
MOLECULAR ION BREAKUP
CARBON ARC DISASSOCIATES MOLECULAR BEAM
HIGH-ENERGY NEUTRALS
TRAPPED ATOMIC BEAM
MAGNETIC FIELD OUT OF PLANE OF ILLUSTRATION
ION GUN PROVIDING MOLECULAR DEUTERIUM BEAM

FIGURE 1.—The concept of molecular ion dissociation in the carbon arc, and the establishment of a closed orbit for the resulting atomic ions.

heating mechanism, and it comes in at right angles midway between the coils, intersecting the arc, so that the atomic ions are trapped in the center of the machine.

The idea, then, is to build up as large a population as possible of the trapped atomic ions, and if on the average they can be held in there for a second or so, they will bounce upon one another and become random in motion. Although we experiment at present with hydrogen because it is easier eventually we would go to deuterium, and under those conditions nuclear reactions should start to take place.

Representative PRICE. Doctor, is this the basic machine that you work with at Oak Ridge?

Dr. SNELL. Yes; this is the DCX machine at Oak Ridge.

Representative PRICE. How large a machine is it?

Dr. SNELL. I can show you a picture (fig. 3.) This is how it looked about a year ago. You see it is 20 feet or so high. The vacuum

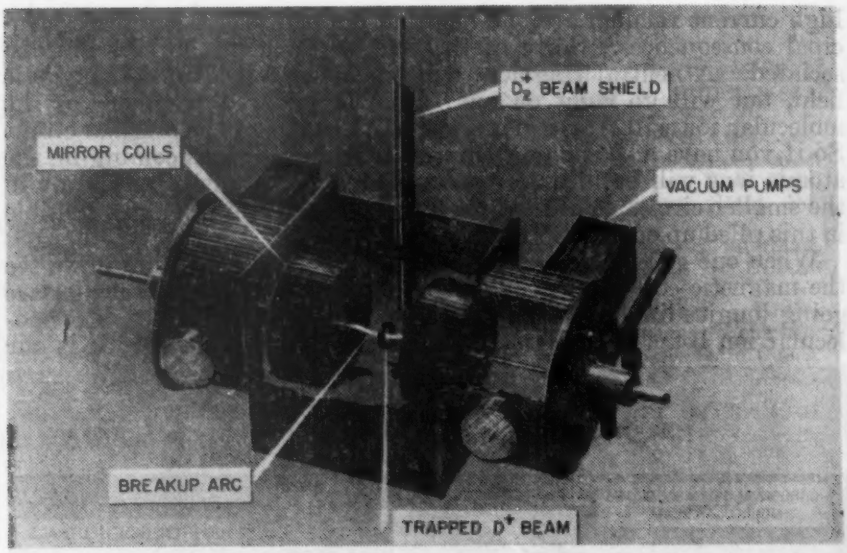

FIGURE 2.—Diagram of the DCX experiment, with magnetic mirror coils, molecular ion injection into the region between them, with dissociation upon a longitudinal vacuum arc.

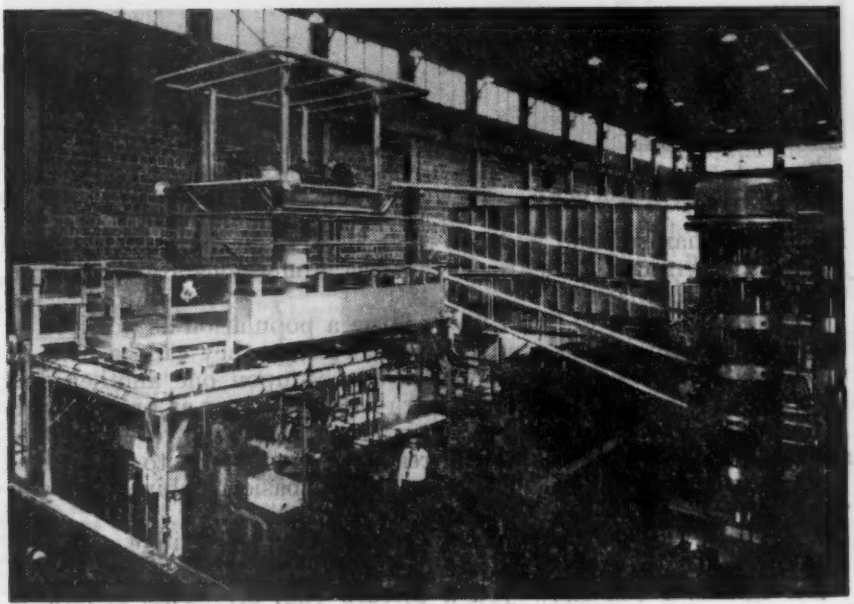

FIGURE 3.—The DCX apparatus as it appeared in 1959.

tank is about 12 feet long. The fact that we inject at 600,000 volts direct current means that some parts get rather large. One has to go to rather large insulators. The 600,000 volt direct current supply is at the right. The ion source and the injection mechanism are on top of the platform, and the vacuum tank looks like a minor part of the equipment, underneath the platform.

Thus, if we could eventually get nuclear reactions to take place they would release nuclear energy. If eventually we could contain the nuclear reaction products, the thing might conceivably start to heat itself up like an ordinary fire. This is the conceptual simplicity and directness of the Oak Ridge approach.

That the molecular injection scheme works can be shown in the next picture (fig. 4). This is an actual photograph taken through a window in the side of the DCX machine. The arc is the bright streak running across the bottom. The trapped atomic ions occupy the circle that you see sidewise standing on edge upon the arc. The molecular ions come in from the front and go out the back, and are not visible. The atomic ions also would not ordinarily be visible, but for photographic purposes we dribbled a little bit of metal powder from the top. It fell down, and as it struck the circulating beam it became incandescent, and we could take a picture of it. So you can see the circulatory population of atomic ions very nicely marked out.

There are about 20 amperes of circulating current, and the molecular ion beam is only 5 milliamperes, so it just doesn't show up. Evidently, however, the molecular injection scheme works in a qualitative way very beautifully.

The question, then, is how are we coming quantitatively? The answer is that in this trapped ring we have trapped 10 to the 13th hydrogen atoms. They live for about a hundredth of a second, and they build up to a density of about 2 billion atoms per cubic centimeter. Two billion is many thousands of times less than are needed to be of thermonuclear interest, so that the question is what limits the density? Why don't the atomic ions accumulate to higher densities? The answer lies in a phenomenon called charge exchange. Although our processes take place in a vacuum, you must remember that the vacuum is not perfect. The trapped atomic ions will sometimes come near one of the molecules of the residual gas. When that happens, one of the electrons on the gas molucule may leave the gas molecule and switch over onto the atomic ion. That turns it into a neutral atom. It no longer is held by the magnetic field and it flies out. So our loss mechanism at present is the one that we foresaw; namely, that all these ions that we so carefully accelerated outside and put into the trap are just flying out pinwheel fashion as a result of the charge exchange.

How do we propose to meet charge exchange? The answer is by going to a conceptual state of affairs called burnout. If you put enough ions into the trap and if you get a good enough vacuum, then the interior part of the ring will be protected by the outer layers, so that gas molecules coming in from all directions will charge exchange before they actually penetrate deeply into the trapped circulating beam. The situation is something like a campfire in an rainstorm. A small fire won't have much chance, but if you build a big dense fire, then the rain can wet the fringes but the core can keep on burning. Of course, it helps if you can arrange to abate the intensity

FIGURE 4.—Photograph of trapped circulating atomic ion current in DCX as revealed by incandescent metal granules. The bright horizontal band is the vacuum carbon arc.

of the rain, too. That is the parallel to getting a better vacuum. All this is fine, and it is what we foresaw some years ago. But then came one feature which we did not completely foresee, simply because the vacuum arcs are new things in scientific experience, and we did not know all of their properties. We have been using the carbon arc almost entirely up to now. The carbon arc contains entirely electrons and carbon ions. The electrons need not concern us here. The carbon ions are multiple charged, but even if they are, there are still two or three electrons left of the normal complement of six electrons. The atomic ions passing through the arc can charge exchange with one of these remaining electrons. So we find an especially large charge exchange loss resulting because the atomic ions pass repeatedly through the arc—perhaps 100,000 times. So no matter how much we improve the vacuum or put in more beam, so long as we keep to that carbon arc, we will not get to burn out. We have to do something about the arc. We have to change its nature.

For that purpose we have been developing other arcs and the most promising is the hydrogen arc. It is promising because there are no electrons attached to hydrogen ions in the arc, and therefore you can have no charge exchange. We find that we can run hydrogen vacuum arcs very well. They carry 300 amperes. The only disadvantage is that you have to feed in a lot of hydrogen. This rather spoils your other requirement; namely, that of a good vacuum. The only way around that is to put on bigger vacuum pumps, and that is what we are doing to DCX right now.

Figure 5 is a picture taken of DCX just last week as it is currently being reassembled after changes. We are putting on four of the biggest vacuum pumps that we can buy in order to be able to work with a hydrogen arc.

Representative BATES. When do you expect to have that completed?

Dr. SNELL. We expect to have a part under vacuum at the end of this week and the remainder of it in about 2 weeks.

Representative BATES. You will have the hydrogen arc, too?

Dr. SNELL. Yes; we have already tried the hydrogen arc in DCX for a few hours, but with the old pumps the vacuum was too poor to get us anywhere.

There is another way of getting over the charge exchange difficulty, but to explain it I have to go back to figure 1. You notice that some of the molecular ions are not dissociated in the arc. When we started this experimentation we measured the efficiency of dissociation and found it to be about 40 percent. This meant that one pass through the arc would probably be enough. Since then experience has shown that under those conditions the arc is rather dirty. If you make matters more sanitary, the dissociation efficiency drops to five or 10 percent. This means that you are wasting 90 to 95 percent of the beam that you put in. In fact, you worse than waste it, because it presents a disposal problem. Then it is obvious, is it not, that you should pass the molecular ion beam through the arc more than once. For example, if you tilt the direction of injection a little out of the plane of the figure, then the beam would spiral outward and pass through the arc a number of times, instead of simply going around and hitting the back of the injector. Some method like that looks at present to be perhaps the most encouraging way for us to go to surmount the charge exchange

FIGURE 4.—Photograph of trapped circulating atomic ion current in DCX as revealed by incandescent metal granules. The bright horizontal band is the vacuum carbon arc.

problem. From the measurements on DCX we know pretty well exactly what to expect. To follow that course we have to build a bigger apparatus because DCX is not large enough to give multiple passes of the molecular ions through the arc. Therefore, we have

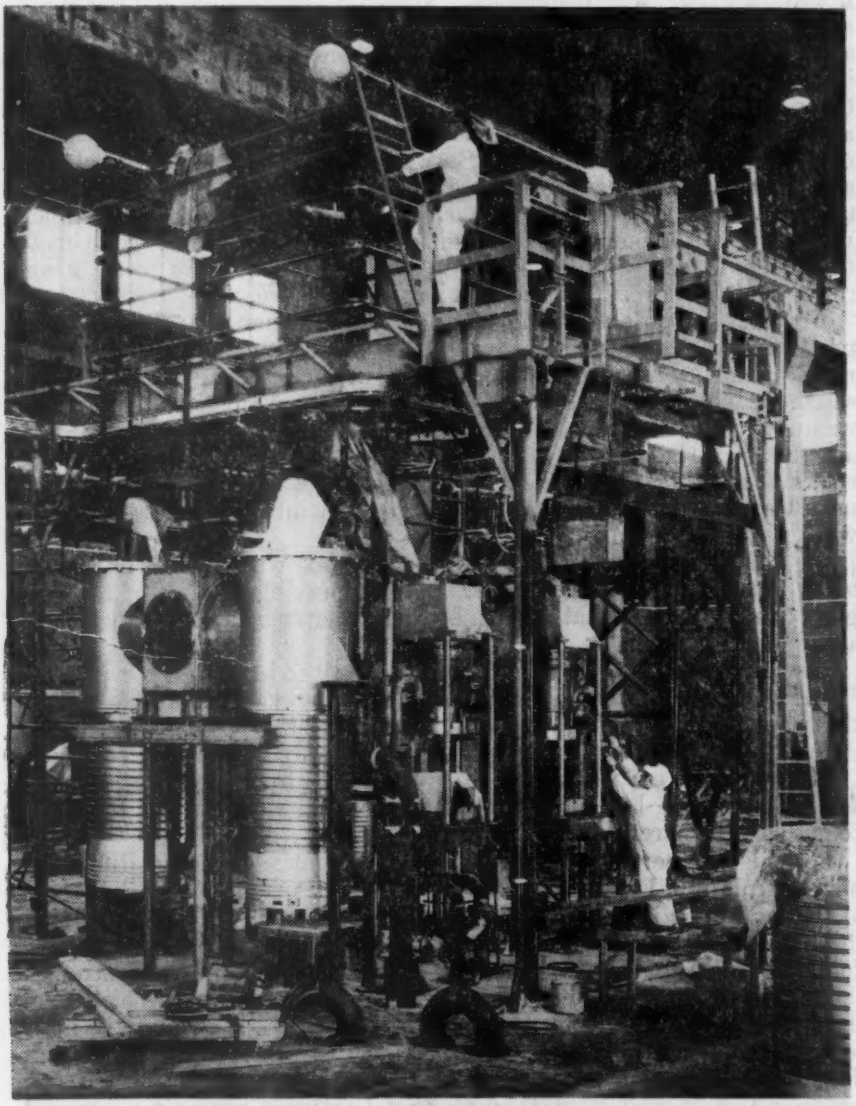

FIGURE 5.—The DCX apparatus as of March 15, 1960, in course of relocation and substantial physical modification.

been discussing with Dr. Ruark and colleagues in other laboratories a second apparatus such as is suggested in figure 6. The magnetic mirror coils and other coils will be arranged to give a carefully designed uniform magnetic field. Molecular ions will be put in at a slight slant through a magnetically shielded injector. The arc will go through the length of the machine. The molecular ions will pass

through the arc 50 or 100 times, and this will use up all of the injected beam, we think. While they are spiraling to the end and back after a reflection, they will be dissociated and the atomic ions will be trapped in the same region. There will be a large amount of mixing of molecular ions with atomic ions. That is an essential feature of this equipment.

In summary, then, I have tried to explain how we have encountered the first hurdle, namely, the charge exchange which we foresaw, and how we think there are at least two ways of getting over it. We don't think we are stopped yet. To push the original DCX as far as burnout in its present course will be difficult, but meanwhile it has been successful in suggesting an alternative mode of approach. All along we have developed our tools and techniques. We are working right at the frontier of technology. Nobody else has generated or handled

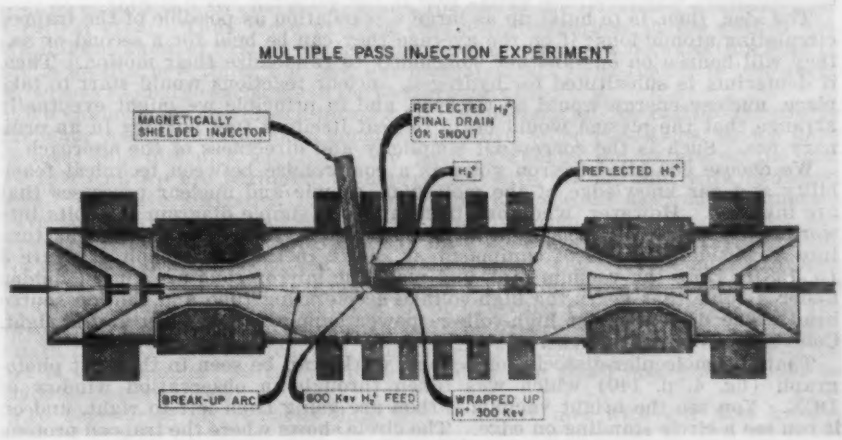

FIGURE 6.—Diagram of proposed multiple-pass experiment.

beams of many milliamperes of ions at energies approaching 600,000 volts. The vacuum arcs are novel. Even in vacuum work we encounter problems of arc and plasma pumping that have not been proven anywhere. So to generate our nuclear fire we have to go right on working, uncovering one by one obstacles that nature may have in store for us, understanding them when we meet them and circumventing them as we can.

Representative PRICE. Thank you very much, Dr. Snell.

(Dr. Snell's prepared statement follows:)

PLASMA ACCUMULATION BY HIGH-ENERGY INJECTION: PRESENT STATUS AND FUTURE PROSPECTS

(By Arthur H. Snell, Oak Ridge National Laboratory, Oak Ridge, Tenn.)

In addressing ourselves to the question of the present accomplishment and future prospect of the Oak Ridge approach to the problem of lighting a nuclear fire, the general remark can be made that we have run into difficulties (some of which were foreseen) but that these difficulties are not fundamental in character; we see ways to overcome them. If with patience and skill we can do so, then we expect to progress until we will doubtless meet with another set of difficulties. We have yet to see the nature of the second set.

To enlarge upon this I have to explain what we are doing. We try to accumulate a hot hydrogen plasma by heating it externally and feeding it into the containment volume of a magnetic mirror. Now we know that if you shoot a

stream of ions into a magnetic field from outside, they will inevitably re-emerge unless you administer some trickery to hold them there. The Oak Ridge brand of trickery is to inject the ions as molecular ions, and to dissociate the molecules within the magnetic mirror, so that the resulting atomic ions are trapped. For thermonuclear purposes one would have to inject deuterium, but at this stage we use hydrogen molecular ions (H_2^+) because they are easier to handle. The external heating consists of an acceleration through 600,000 volts, and the dissociation scheme is shown in figure 1 (p. 137). The molecular ions follow a curved path in the magnetic field (all of this is in vacuum), and at a certain point they are made to pass through a high-current vacuum arc. Here some of them are dissociated, and the H^+ ions so formed travel in a circle with half the radius of that followed by the H_2^+ ions. Any of the latter that are not dissociated pass on out of the field. Figure 2 (p. 138) brings us a step nearer to reality. One sees the two mirror coils with the arc running lengthwise through them, and the molecular ion beam being brought in perpendicularly at the center, aimed so as to intersect the arc, and forming atomic ions that are curled into tighter orbits, trapped at the center of the apparatus, and orbiting continuously until something happens to them. All of this is carried out on a continuous, direct-current basis; this is not a pulsed apparatus.

The idea, then, is to bulid up as large a population as possible of the trapped circulating atomic ions; if on the average they can be held for a second or so, they will bounce on one another sufficiently to randomize their motion. Then, if deuterium is substituted for hydrogen, nuclear reactions would start to take place, nuclear energy would be released, and in principle we might eventually arrange that the plasma would begin to heat itself as fuel burning in an ordinary fire. Such is the conceptual simplicity and directness of the approach.

We choose 600 kilo electron volts as a compromise between technical feasibility and our knowledge of the molecular, atomic and nuclear processes that are involved. However, when one translates the simple diagram of orbits into working hardware, the 600-kilovolt feature makes the DCX experiment turn into a considerable pile of equipment as shown in the photograph of figure 3 (p. 138). Here the vacuum tank and magnetic mirror coils are almost hidden under a table that bears the high-voltage accelerating tube and the ion source head. The direct-current high-voltage power supply can be seen at the right. Control racks are in the rear.

That the molecular-dissociation scheme works can be seen in the next photograph (fig. 4, p. 140) which was taken through an observation window of DCX. You see the bright vacuum carbon arc going from left to right, and on it you see a circle standing on edge. The circle shows where the trapped protons are. Normally, they would be invisible, but for photographic purposes metal powder was dusted down from above. When the granules hit the circulating proton beam, they became incandescent, and so you see the proton population nicely marked out. The circulating proton current amounts to several amperes; the incoming and outgoing molecular ion beam, on the other hand, was only about 1 milliampere, and so is invisible.

Qualitatively the scheme therefore works as planned, and the next question is, "How far have we progressed quantitatively?" The answer is that with an incoming H_2^+ beam of 5 milliamperes, we have trapped 1.1×10^{12} atomic particles into the circulating orbit, in which on the average they stay for a hundredth of a second, and circulate about 100,000 times before they are lost. The resulting trapped ion density is 2 billion atomic ions per cubic centimeter. This is many thousands of times less than any density that would be of themonuclear interest, so the question arises: What prevents the density from increasing further? Why does it stop at 2 billion per cubic centimeter?

The answer lies in a phenomenon known as charge exchange, and this was to be our first major difficulty, as we foresaw. The point is that although our trapped atomic ions are in a vacuum, that vacuum is not perfect, and with residence times as long as a hundredth of a second it is likely that an ion will at some time pass close to a neutral gas molecule. When this happens, one of the electrons belonging to the gas molecule can transfer to the atomic ion; the latter then becomes a neutral hydrogen atom, and is no longer retained by the magnetic field. Hence these neutralized atoms that we had gone to a lot of trouble to accelerate are continually flying pinwheel fashion out of the trapped ring, and this is our major loss mechanism. The other production of the charge exchange is a slow molecular ion; it will soon scatter into a direction parallel with the magnetic field, and when this happens it can pass unhindered out through the throat of one of the mirror coils, and can be pumped away.

How do we propose to overcome charge exchange? By resorting to a conceptual state of affairs called burnout. By reducing the amount of background gas present, and by injecting a heavier current of ions, it should be possible to build up the density of trapped atomic ions to the point where a neutral gas molecule cannot penetrate far into the region occupied by the trapped ring before it is charge exchanged. When this state of affairs is reached, the inner regions of the ring will be shielded by the outer regions. One might liken the situation to a campfire in a rainstorm; a small fire has little chance, but in a big, dense fire the raindrops may dampen the fringes while the core keeps burning. Of course it also helps if you can arrange to abate the intensity of the rain, and that would be the counterpart of improving our vacuum conditions.

All of this has been in our plans for some time back, but now I come to a factor the seriousness of which had not been completely foreseen. Let us consider again the vacuum arc that is used to dissociate the molecular ions. So far, we have used almost exclusively a 300-ampere carbon arc for this purpose. The carbon atoms in the arc are all ionized, usually multiply so, but nevertheless each carbon atom has two to five of its normal complement of six electrons still present. Our high-speed trapped atomic hydrogen ions pass repeatedly through the arc, and as they do so they can, by charge exchange, rob one of the remaining electrons from a carbon atom. Thus there exists a charge-exchange loss in the carbon arc. We did not know at first of the importance of this process, because this kind of arc is itself a new thing, but now our measurements show that it is just about as serious as charge exchange in the background gas.

We cannot hope to achieve burnout in the presence of the carbon arc charge exchange loss no matter how much we improve other conditions, so we have to meet this difficulty squarely. We have to change the nature of the arc. To this end, we are beginning to experiment with lithium and with hydrogen arcs. We have succeeded in running both, but special electrode techniques have to be worked out in each case. The hydrogen arc looks best for our purpose, because ionized hydrogen has no atomic electrons and therefore the arc charge-exchange loss should entirely disappear. Our experimental hydrogen arcs are beginning to look very good indeed. They do, however, impose a severe vacuum pumping problem, because a lot of hydrogen gas has to be fed into the vacuum system in order to support them. The experimentation with a hydrogen arc in DCX has just started, and it is too early to report results. The lithium arc is still younger; so far we have run only one small one, but it could be made to behave nicely.

To summarize my remarks thus far: We have met with our first hurdle, that of charge exchange, and we have not yet surmounted it. To do so in the present configuration of DCX we need a stronger input of ions, a better vacuum, and a better arc. We think that all three of these requirements can be met in some measure, but the question is: Can they be met adequately to get us to the stage of burnout? Our calculations show that instead of 2 billion ions, we need 600 billion ions per cubic centimeter in our trapped atomic ion current for hydrogen to achieve burnout, and 2 trillion per cubic centimeter for deuterium at the same injection energy. Furthermore, this assumes that our ions do not spread into a larger occupied volume, as well they might. To reach these ion densities is certainly going to be difficult, and will require skillful experimentation of high order, but we intend to go in that direction as far as we can, and to that end we are right now rebuilding DCX with four of the biggest vacuum pumps that we can buy attached to it. In fact, the vacuum pumps dwarf the remainder of the equipment, which is only appropriate and necessary from our point of view. Figure 5 (p. 142) is a photograph of DCX as it appeared last week, and you see two of these vacuum pumps in place. The old vacuum pumps look puny and insignificant in comparison.

You may have noticed that I say that burnout will be difficult to reach in DCX in its present configuration. That reservation brings me to another line of attack which we have discussed among ourselves a great deal lately, and which looks probably more hopeful, but to explain it I must go back to figure 1. You notice that not all of the molecular ions are dissociated as they pass through the arc. When DCX was first built, experiments had shown that about 40 percent of the molecular ions were dissociated, and to keep the apparatus simple it was decided to settle for that. Since then, we have found that although 40 percent dissociation is indeed easily attained, nevertheless it is reached only under rather dirty arc conditions. There is a lot of undesirable carbon flying around. If we clean up the arc, obtaining what are obviously more sanitary operating conditions, the dissociation efficiency drops to 5 or 10

percent. In other words, 90 to 95 percent of our injected molecular beam is being wasted; indeed, it is worse than wasted, for it presents a severe disposal problem.

Surely, then, it is obvious that we will be better off if we could arrange to pass the molecular ion beam through the arc not only once, but several or many times. We have played with conceptual designs, and think that it should be possible to make 50 or 60 passes, and dissociate essentially 100 percent of the molecular ion beam. Furthermore, it is fairly easy to predict from present DCX results just what conditions would have to be met with regard to arc, vacuum, and injected beam current to get us substantially beyond our present densities of 2 billion ions per cubic centimeter. Let me state the answer in a simple way: If we keep to the present carbon arc, with its present disadvantages, and if we keep to the present vacuum conditions, which are by no means wonderful, then an injected molecular ion beam of about 100 milliamperes should put us into the vicinity of burnout. This magnitude of ion current does not scare us in the least. We think that we can generate such a beam; in fact, we already have an ion source that is more than adequate. Nobody has yet handled a beam of a tenth of an ampere at 600,000 volts, but we are going to try during the next few months. Furthermore, if we improve our arc or our vacuum conditions (which we also are quite sure that we can do) then the injected beam current requirement will become more modest.

To pursue this attractive course of action requires a new magnetic containment apparatus, because DCX is too small to lend itself to a multiple-pass configuration. In my last figure (fig. 6, p. 143) I show a sketch of the kind of thing we are thinking of. The vacuum tank is about 3 feet in diameter and 27 feet long; you see the magnet coils surrounding it, the arc running from end to end, and the molecular hydrogen beam led into the vacuum through a magnetically shielded injector, aimed so as to cause a spiraling of the injected ions with repeated passes through the arc. You see also the orbits of the trapped atomic ions at smaller radius, and the mixing of the two kinds of orbits.

The proposed multiple-pass experiment has been under review by Dr. Ruark and our scientific colleagues at other Sherwood laboratories for the last 2 weeks. We have had generous help and constructive comments from all.

Perhaps I have now said enough to explain how we have encountered difficulties, and why we don't think we are stopped yet. To push DCX far in its present course will be hard, but meanwhile DCX has been successful in suggesting to us a modified approach that looks easier. We have to develop our tools and techniques as we go along. We are working right at the frontier of technology; nobody else has generated or handled beams of many milliamperes of ions at energies approaching 600,000 volts; the vacuum arcs are novel in scientific experience; and even in vacuum work we encounter new features of arc and plasma pumping that have not yet been proven anywhere. So, to generate our nuclear fire, we have to go right on working, uncovering one by one whatever obstacles nature may have in store for us, understanding them when we meet them, and circumventing them as we can.

Representative PRICE. The concluding witness this morning will be Dr. Melvin B. Gottlieb, of Princeton University.

STATEMENT OF MELVIN B. GOTTLIEB,[1] PRINCETON UNIVERSITY

Dr. GOTTLIEB. Thank you, Mr. Chairman. I am going to be describing the stellarator program at Princeton under the leadership of Lyman Spitzer, Jr.

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[1] Born: Chicago, Ill.: May 25, 1917.
Education: B.S., University of Chicago, 1940; Ph. D., University of Chicago, 1950.
Professional experience: Instructor (electronics), University of Chicago, 1941–44; project leader (radar countermeasures), Harvard University; instructor in physical sciences, University of Chicago, 1945–56; project leader (V–2 rocketborne ionosphere measurements), University of Michigan, 1946; research assistant, University of Chicago, 1947–50; assistant professor of physics, State University of Iowa, 1950–54; associate director and head of experimental physics (controlled fusion), Project Matterhorn, Princeton University, 1954 to present; chairman of Plasma Physics Division of American Physical Society, 1959 to present.
Societies: American Physical Society, Sigma Xi, Phi Beta Kappa.
Publications are mainly in the fields of plasma physics and cosmic ray physics.

The basic problem, as you understand, is the containment of the hot gas in the magnetic field and the holding of this gas for a sufficiently long time. You can create a magnetic field in a long tube. Suppose we have one in a given direction; it keeps the particles from moving at right angles to the field, but they can move along the field freely. So you have end effects, and you lose particles out the end.

The mirror attempts to close the ends and it does close them to an appreciable extent, but not fully. You might try to say, "Let us eliminate the ends entirely by simply turning the tube around on itself as a torus." The difficulty here is the fact that the particles get thrown outward.

The stellarator approach is to try to confuse the particles so they won't know which way is out. It happens this way: The particles start moving along the field. They move out as they go along and, as they move out, they go along the field and come to the other end, but now out turns into in. So they essentially don't know which way is out. We partially fool them. Therefore, in theory the plasma can be contained inside the magnetic field in a sort of delicate balance like a man on a tightrope. This balance is a very delicate one. In this figure 8 type of stellarator (see p. 148) it is possible for them to fall out, like the man on a tightrope whose arms are tied together. The slightest disturbance will do this. We try to give them a balancing rod to give more possibility of staying on the tightrope. In other words, we try to create a stable equilibrium. In order to do this we modify that figure 8. Instead of twisting the tube itself, we twist the field inside the tube.

You may be able to see some spiral copper wires winding around the tube. They cause the field inside to twist around, still fooling the particles, but it turns out they have the additional advantage of making the system more stable, giving the man on the tightrope a balancing bar so he does not fall off as easily.

How do we heat the gas in the tube?

Representative BATES. Why didn't you use the figure 8 in conjunction with the new concept?

Dr. GOTTLIEB. It turns out some of our experimental devices do just that. I will show one in a moment. Which system or combination one uses is just a matter of computation to see which one is better. In principle one can use many different methods of heating the gas inside the system. We chose initially the simplest way by starting with neutral gas as you get it out of the tank and heat it up by passing a current through it. This will work up to about a million degrees in principle, but as the gas gets hot its conductivity improves, so you don't heat it very much by passing a current through it. Therefore, some other method of heating is needed to get to still higher temperatures. We have methods which we have described in principle and tried out to some extent involving alternate compression and expansion of the plasma for further heating.

While this method of heating is simple, it is also theoretically subject to a number of instabilities. Whether these instabilities will actually be troublesome one needs experiments to ascertain, because the theory represents a vast oversimplification of the actual physical problem.

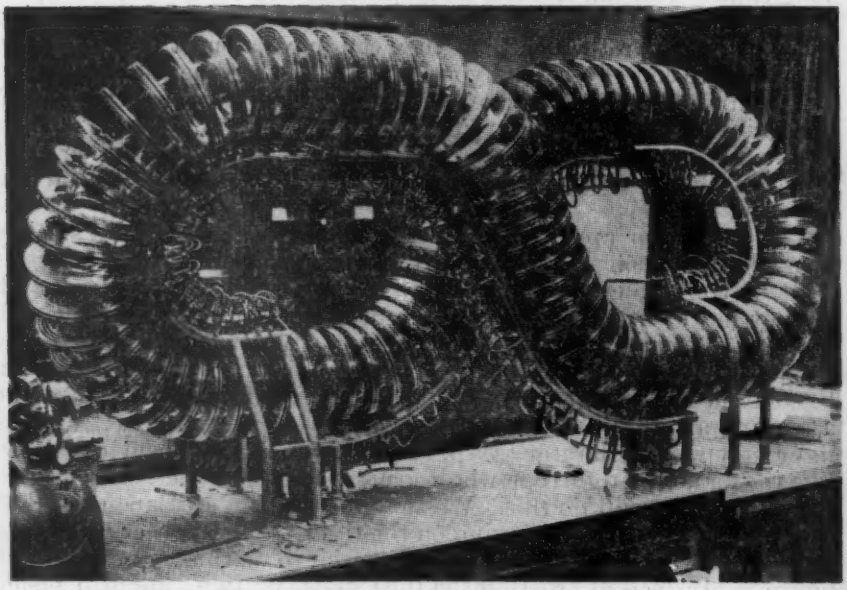
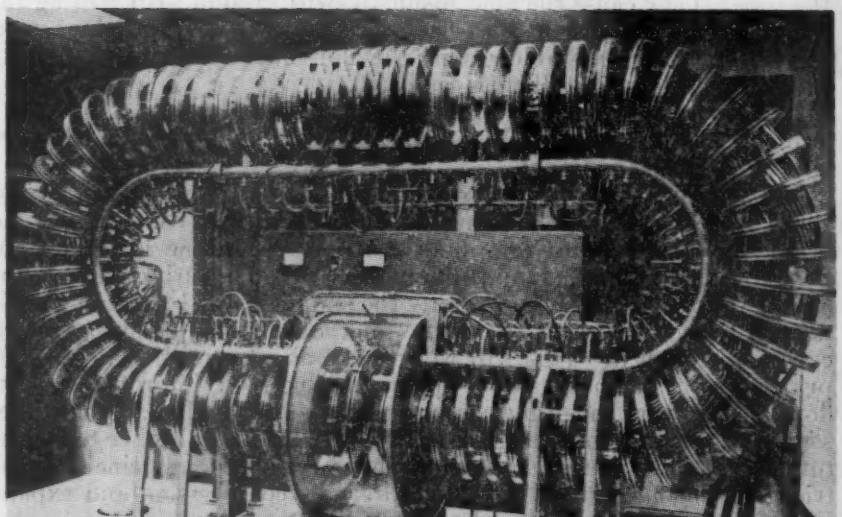


Figure 8 stellarator.



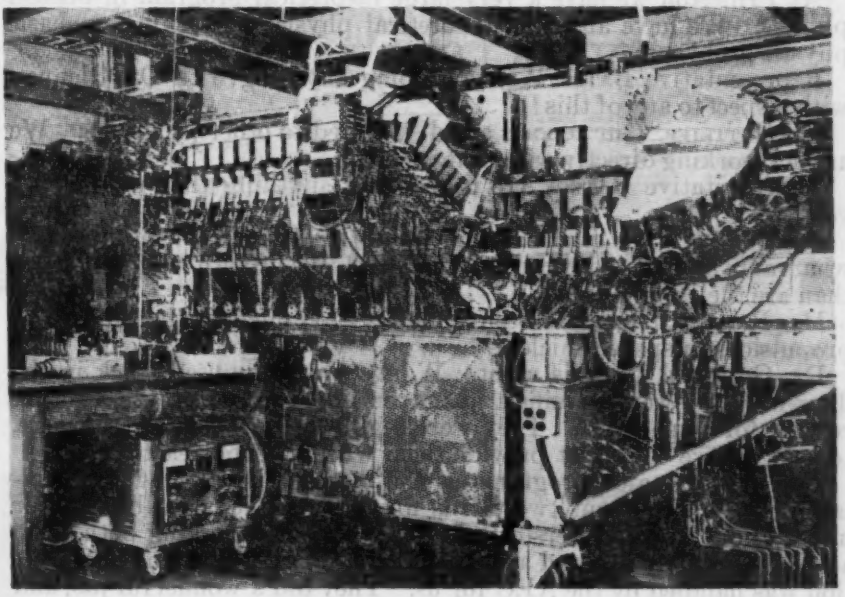
Stellarator with field twisted inside torus.

We have five stellarators in operation. Below is a picture of one of them, the B–3 stellarator. This uses, as has already been suggested, a combination of the figure 8 and the twisted field. These five stellerators have various principal functions. There are additional small experiments going on, using magnetic geometrics different from that of the stellarator.

Mr. RAMEY. How many people do you have?

Dr. GOTTLIEB. We have roughly 100 professional people.

What about the observations that have been made in these systems? Our early observations—and by the way, the early observations are aimed at finding out whether these instabilities which are predicted as possibilities do exist—our early experiments found that the gas heated roughly as expected, but then there followed vast influxes of



Model B–3 stellarator.

impurities so there were more impurities than original material. We had to go to very extensive steps involving new technology in vacuum systems to try to improve this impurity situation. As a result, we have been able to get some more details about what is going on. We find that the gas moves to the wall in about a ten-thousandth of a second in general. We have been trying to find out the main reasons for this phenomenon which we call pumpout.

One thing which should be emphasized in this field is that we do not know enough of the basic physical laws. The astrophysicists tell us that about 95 percent of the universe consists of very hot gases called plasmas. But such plasmas are very uncommon on earth. Here on earth the temperatures are rather low. We simply have not had these kinds of physical experiences, which would teach us the nature of the basic physical laws. The early work on this subject was largely done by the astronomers, the astrophysicists, trying to understand what was going on in the stars. Indeed, our director, Lyman Spitzer,

is an astrophysicist. We are going to have to know a lot more about plasma physics before we really know how to handle the fusion problem.

All of the programs are engaged to a very considerable extent in trying to find out the basic laws that govern the actions of plasmas. They are very complicated, much more complicated than our ordinary physical laws. By analogy, suppose we know all about how a man acts when confronted with various situations, a man standing alone. This won't tell us how he will act when standing in a mob. The laws are different. They are changed in some subtle way. In a similar manner a plasma consists of a huge number of particles. We know how the pairs interact. When they get into a large crowd, they do different things than we have predicted.

At Princeton we have a very large theoretical program in plasma physics. We have about 12 theoretical physicists working on basic physics problems.

Representative BATES. Are you doing anything with Dr. Glennan with respect to any of this?

Dr. GOTTLIEB. Our reports are distributed to other programs. We are not working directly with that program.

Representative BATES. Are they doing anything that might help you?

Dr. GOTTLIEB. To some extent, yes. I think our problems are somewhat more specialized because of the involvement with magnetic fields than are theirs.

Mr. RAMEY. There has been some research done on thermonuclear propulsion for space vehicles.

Dr. GOTTLIEB. That is right. We were very amused, back in the days when we were classified, about certain proposals to use a fusion reactor for a spaceship. They were highly in favor of it. We were, too, except we could not provide them with a fusion reactor.

One of our major programs at the moment is the model C Stellarator. This is about two-thirds finished, and it is really a major undertaking for a university. It is a major undertaking both from a building standpoint and apparatus standpoint. Building construction was handled by the AEC for us. They did a wonderful job, and saved us from many headaches. The largest building is for the motor generators. The experimental area is in a neighboring building. The apparatus, too, is quite complex, as I can illustrate.

Here is a schematic diagram of the device. This is a toroidal figure with the slowly spiraling windings around it to give it the stable magnetic configuration.

These are coils; the forces in the coils are very large. They must be built in a very massive manner. I will show you what one of them looks like. These coils are now in production. The first one has arrived at our laboratory. Some of the other complexities can perhaps be best seen in this picture which shows the preparation of the stabilizing windings. This is going on at the vendor's plant, the Allis Chalmers Co., in Milwaukee. Here are the stabilizing windings being formed on the jig preparatory to final bracing on the end. This is quite a task.

Mr. RAMEY. What is the size of your torus?

Dr. GOTTLIEB. It will be 7 feet in diameter. The tube that goes inside will be 8 inches in diameter.

Representative PRICE. You are talking about the Model C?

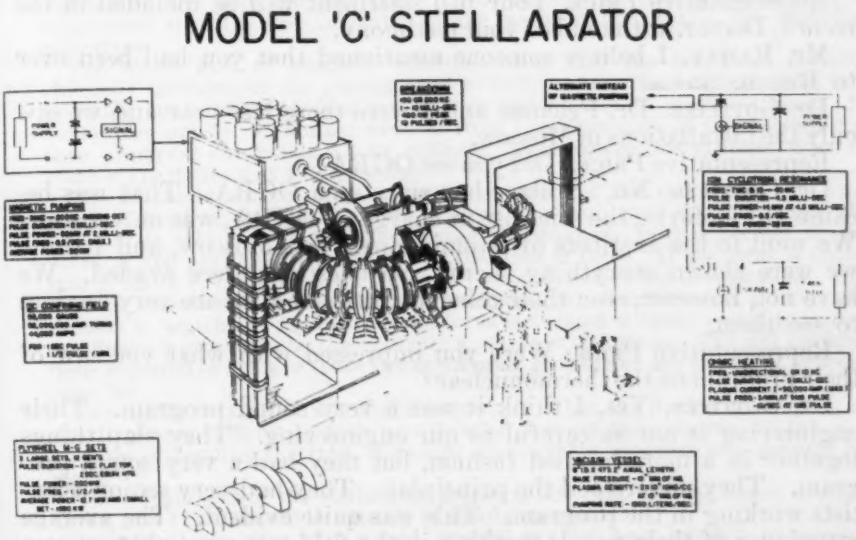
Dr. GOTTLIEB. Yes. I should point out that this is not a prototype reactor or anything of the sort. In fact, from its design it is inconceivable that it could be used for such purposes. It may be used only for experimental work, trying to find out about basic plasma behavior.

Mr. RAMEY. But it is a pretty good size compared to some.

Dr. GOTTLIEB. It is very large size. Size is not in itself a desirable property for an experimental gadget because it makes things much more inflexible, much harder to change. If you can avoid making things large, you are that much better off.

Representative VAN ZANDT. How much money will it involve?

Dr. GOTTLIEB. The buildings themselves cost about $10 million. May I beg the question for just a moment? When you say the cost,



there are two aspects to the cost. One is the cost of providing power and the external facilities that will be used to power this experiment and many others in the future. The other is the cost of this gadget. The cost of all the facilities together is $26 million, of which probably $20 million is of the permanent facility type which would be used with further gadgets to be built here, some of which are already on paper. That is, on the backs of envelopes.

Senator AIKEN. Why do you call it model T?

Dr. GOTTLIEB. Model C. I am sorry we didn't call it model T.

Senator AIKEN. Model T was the first general application. I wondered if you were that far advanced.

Dr. GOTTLIEB. Definitely not, although this has replaceable parts. Back in the naive days when we first got going, we said, "We know we don't know enough about the subject but perhaps what we don't know won't bother us." This was indeed naive—we had four stages of experiments. A, B, C, and D, where D was the prototype. C turned out to be about the scale of this device so we just called this C in our original nomenclature. We hope we, ourselves, will never

have to build a D. We hope we can solve the problems and let somebody else do that.

Representative PRICE. Does that complete your statement?

Dr. GOTTLIEB. Yes, sir.

Representative PRICE. Are there any questions of Dr. Gottlieb?

Mr. RAMEY. Pursuing the line of questioning on the size of your devices, a couple of years ago when the British went to their ZETA device, and it looked like it was working and making a thermonuclear reaction, there was quite a discussion of scaling up to larger sizes. Has thinking changed in that regard in the last year or two?

Dr. GOTTLIEB. We like to solve the problems insofar as possible by the smallest gadget which will do the job. If ZETA had been successful, we would have decided that size alone is an essential quality. But ZETA did not succeed. There are certainly some advantages in size and I have a whole list of them in the prepared paper.

Representative PRICE. Your full statement will be included in the record, Doctor, at the end of your testimony.

Mr. RAMEY. I believe someone mentioned that you had been over to Russia.

Dr. GOTTLIEB. Dr. Frieman and I were there last year and we saw only the installations in Moscow.

Representative PRICE. Did you see OGRA?

Dr. GOTTLIEB. No. Spitzer has since seen OGRA. That was because Dr. Golovin, the scientist in charge of OGRA, was on vacation. We went to the Institute of Atomic Energy at Moscow, and I think we were shown everything there. No questions were evaded. We have not, however, seen their other facilities and we are very anxious to see them.

Representative PRICE. Were you impressed with what you saw of their program on the thermonuclear?

Dr. GOTTLIEB. Yes, I think it was a very sound program. Their engineering is not as careful as our engineering. They slap things together in a more hurried fashion, but they had a very sound program. They understood the principles. They had very senior scientists working in the program. This was quite evident. The average experience of their people working in the field was somewhat greater than in this country.

Senator AIKEN. Did the Russians show any reluctance to show you what they were working on?

Dr. GOTTLIEB. When they finally decided, after a week of discussion, to allow us to visit the Institute of Atomic Energy, they seemed to show us everything, and answer all of our questions. There are several areas other than Moscow in which interesting work is being done which I don't believe any Americans have as yet seen. We would like very much to visit these areas, because we know what kind of work is going on there. We have talked to scientists from these laboratories. They were not at all reticent to speak about their work. We have not seen these installations.

Representative PRICE. Thank you very much, Doctor. I want to thank you and all the other witnesses who testified both yesterday and today, and the other scientists who did not testify, but who have shown their interest in this hearing by being present here, both yester-

day afternoon and this morning. We deeply appreciate your interest in these hearings.

The committee will stand in recess until tomorrow morning at 10 o'clock. We will meet in the same room and the session then will be on the subject of advanced reactor concepts.

(Dr. Gottlieb's prepared statement follows:)

THE PRINCETON UNIVERSITY PROGRAM ON CONTROLLED THERMONUCLEAR REACTIONS

(By Melvin B. Gottlieb)

The question basic to the entire controlled thermonuclear reactor program is whether adequate containment of a hot gas by a magnetic field is possible. If it is possible, then it would still remain to be ascertained how best one could accomplish this containment while at the same time heating the gas to the very high temperatures in such a manner as to avoid major disturbances which could interfere with confinement. This talk will deal almost exclusively with one particular approach, the stellarator, on which the Princeton group under Lyman Spitzer, Jr. has been concentrating. I will describe first the basic ideas of the stellarator, then the present status of the research, and finally, the immediate objectives.

The whole idea of magnetic confinement is that the hot (and therefore charged) particles of the plasma tend to follow along the magnetic field lines, provided that this field does not change too rapidly in space or time. The stellarator concept of Spitzer evolved from considerations of the following nature. Suppose one builds a long cylinder. Somewhere this cylinder must end and here particles may freely escape.

The magnetic mirror offers a partial stoppage of the end leak. One might think of bending the cylinder into a torus or donut, thus avoiding ends. Unfortunately this is not adequate because the magnetic field is necessarily nonuniform and particles drift out across the lines of force. If the torus is twisted into a figure 8, the direction "outward" becomes somewhat meaningless. Equilibrium containment is theoretically now possible. Furthermore, a later modification in which the field inside the torus is twisted without resort to twisting the figure 8 proved to be theoretically a stable equilibrium. The coil configurations required to achieve these two stellarator geometries are shown in figures on page 148.

The disadvantages of a stellarator are: (1) that the device is complex, and (2) it is very difficult to take into detailed account theoretically all the consequences of bending the tube, although considerable progress has recently been made.

The advantages of the stellarator geometry are that (1) end losses are eliminated, and (2) it should be capable of steady operation.

In general, we start with a cold, neutral gas and heat initially by passing a current through the gas—so-called ohmic heating. As the temperature rises the conductivity increases so that additional current is needed for heating. However, it is known (both experimentally and theoretically) that the current must not be allowed to rise above a critical level or the system is subject to a kink instability. Therefore, ohmic heating is only useful up to roughly a million degrees.

While indeed simple to apply, there are theoretically a number of instabilities which could take place during ohmic heating even at small currents. The seriousness of these effects cannot be determined theoretically. Experimental investigations of this point have therefore occupied a major portion of the experimental program.

For heating the plasma above a million degrees, it has been intended that magnetic pumping would be applied, in which the plasma is alternately compressed and expanded by means of an oscillating axial magnetic field superimposed on the steady confining magnetic field. This magnetic pumping must be done at some naturally resonant frequency of the system. While a number of different types have been studied theoretically, the particular process of ion cyclotron heating has been emphasized.

Other heating methods could be used in stellarators, similar to those mentioned in previous talks, such as compression, shocks, particle injection, etc., but we have not yet made use of these methods.

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As to the present status, there are at Princeton five small stellarators, each used for somewhat different but complementary studies. The B–3 stellarator is shown in figure on page 149. The 2-inch-diameter stainless steel discharge tube is in the form of a figure 8, about 22 feet in length. The magnetic field is pulsed once a minute at fields up to 50,000 gauss. It is equipped with an ultrahigh vacuum system and has auxiliary stabilizing (helical) windings. It has been used largely for ohmic heating and pumpout studies. Among the measuring techniques applied are:

1. Microwave measurements of average plasma density.
2. Microwave radiation measurements to give some measure of runaway electrons.
3. Spectrographic measurements of ion-temperature plasma motion, rough electron temperatures, impurity levels.
4. Magnetic probe measurements of plasma motions.
5. Streak camera measurements of plasma motions.
6. Langmuir probe measurements of electron density, electron temperature, plasma space potential, and electric fields.
7. Mass spectroscopic measurements of impurity levels.

In the early phases of experimental work on ohmic heating, it was found that the gas did heat roughly as expected; but as the temperature increased, large amounts of impurities flooded into the discharge, rapidly cooling the system and obscuring the detailed processes. It was not until great pains had been taken to radically improve the vacuum systems (by a factor of 10,000), involving major advances in vacuum technology, that more details became clear. We now know that as the gas heats, the particles are lost to the walls in relatively short times (of the order of one ten-thousandths of a second). The reasons for this are not entirely clear and are apparently several. One of the causes is runaway electrons—electrons which escape from the body of the distribution. This effect is observed under conditions where it would be expected theoretically. Another possible cause is ion oscillations induced by the drift motion of electrons. During the past year it has been found that "pumpout" apparently is not due to hydromagnetic instabilities. After the ohmic heating current is turned off, the confinement time becomes longer by a factor of about a hundred. However, the plasma is quite cold at this time, presumably due to cooling by impurity radiation.

Impurities continue to be a major problem. In the past year, although we have reduced the level of impurities considerably (and we can now measure impurity levels much more reliably), at the end of a few milliseconds carbon and oxygen atoms even now constitute almost 10 percent of the number of hydrogen atoms initially in the discharge tube. It is very difficult to heat the gas in the presence of any substantial amount of impurity because of the tremendous loss due to radiation. From the standpoint of a thermonuclear reactor, of course, it would be essential to lower the impurity level by a factor of at least 100. The impurities also make it more difficult to design critical experiments; for example, it would be very desirable to measure the intrinsic confinement time of a stellarator plasma in the absence of ohmic heating. Impurities, however, cool the plasma very rapidly, and it is then recombination of electrons and ions rather than losses of particles across the magnetic field which determine the plasma duration time.

As to program objectives, first and foremost is a better understanding of pumpout. In fact, this statement might well apply in general to basic plasma physics. This field is a very complex and difficult one. On the theoretical side, considerable progress has been made, but because of the complexity, it is necessary to badly oversimplify the problems in order to effect solutions. Only very gradually are the more complex problems being solved. At Princeton, there are 12 theoretical physicists—a very large effort—concentrating on theoretical plasma physics.

Experimentally, too, it is essential to study—in as fundamental a manner as possible—the mechanisms whereby the plasma particles may get across the magnetic field. The Etude stellarator is now concentrating on probe measurements of plasma turbulence as related to pumpout.

With respect to the reduction of impurities, there are now three activities underway which should effect a substantial improvement. A new mercury ultrahigh vacuum system, completely free of oil, will be installed on B–3. A divertor—a device for removing the outer layer of the plasma—is being readied, and cleaner gas sources will be used.

Another very important program objective is the exploration of alternative heating methods. One of these methods, as was previously indicated, is ion cyclotron resonance heating. It has been demonstrated by T. Stix that initial heating of a plasma is possible using this method instead of ohmic heating. It remains as an important objective to ascertain whether pumpout takes place. Thus far the experiments have not been performed on clean enough systems to make pumpout measures meaningful. Feasibility studies on other heating methods are now being made, and it is probable that experimental work will be underway during the next year.

The model C facility is now about two-thirds complete. This facility provides the space and energy storage to make possible considerably larger scale experiments. The first of these experimental devices is now being assembled and is roughly four times as large as present experimental devices in all dimensions. Among the advantages of this device are the following:

1. Pumpout may be slower. In fact, a comparison of the rates as the size changes provides basic information on plasma diffusion.
2. The lower surface-to-volume ratio should decrease the impurity levels. This, combined with slower pumpout, would substantially increase plasma duration times and, incidentally, temperatures.
3. The device will include provision for high modulation magnetic pumping as an alternate heating scheme, and will be sufficiently large that particle injection would be conceivable.
4. The larger scale means that better diagnostic information will be available—e.g., more light and relatively better density resolution.

The buildings are now complete. The machine framework and vacuum system are complete. Coil fabrication is now progressing rapidly. Initial operation toward the end of the calendar year is expected.

(For possible application of thermonuclear reactions to space propulsion a description may be found in a report by Milton V. Clauser of Space Technology Laboratories, Los Angeles, Calif. Rept. No. PRL–8–04, June 2, 1958.)

(Thereupon at 12:05 p.m., Wednesday, March 23, 1960, a recess was taken until Thursday, March 24, 1960, at 10 a.m.)

FRONTIERS IN ATOMIC ENERGY RESEARCH

THURSDAY, MARCH 24, 1960

CONGRESS OF THE UNITED STATES,
SUBCOMMITTEE ON RESEARCH AND DEVELOPMENT,
JOINT COMMITTEE ON ATOMIC ENERGY,
Washington, D.C.

The subcommittee met at 10 a.m., pursuant to recess, in room P–62, the Capitol, Hon. Melvin Price (chairman of the subcommittee) presiding.

Present: Representatives Price, Holifield, Van Zandt, Hosmer, Bates, and Westland; Senators Anderson, Pastore, Gore, Hickenlooper, Dworshak, and Bennett.

Also present: James T. Ramey, executive director; George F. Murphy, Jr., professional staff member, and Edward J. Bauser, technical adviser, Joint Committee on Atomic Energy.

Representative PRICE. The committee will be in order.

This morning in our third session on Frontiers in Atomic Energy Research, we will cover advanced reactor applications. We have with us today the leading people in the United States in the nuclear reactor field. We look to them for leadership on this horizon.

First, some of our scientists will present to us their ideas on specific future application of advanced reactors, and then we have scheduled a panel discussion by a distinguished group of pioneers in the reactor field.

Our first witness this morning is Dr. Bernard Spinrad of our Argonne National Laboratory. It is particularly appropriate for us to lead off with a witness from the Argonne Laboratory, since many of our most significant advances in the field of reactors have come out of Argonne. Such important work as submarine reactors, breeder reactors, and boiling reactors came out of this Laboratory. In addition, many of the leading experts in the industrial reactor field are alumni of the Argonne Laboratory.

It is a pleasure to welcome you as the leadoff witness this morning, Dr. Spinrad.

STATEMENT OF BERNARD I. SPINRAD,[1] ARGONNE NATIONAL LABORATORY

Dr. SPINRAD. Thank you, Mr. Chairman.

I have a chart which I see is being set up which I will use to illustrate this presentation.

(The chart referred to follows:)

Item	Feature	Competitor	Application	Comment
1.	Low fuel cost as mined.	Thermonuclear, fossil fuels.	Central station power. Normal industrial and residential heat.	Fission energy technically proven. Cost and availability good enough in principle.
2.	Compactness of fuel.	None	Special remote power, especially space stations.	Reactor and generator must also be compact, but no refueling problem.
3.	No oxidant needed.	Electricity	Submarine (ships and underwater stations). Refractory metal working.	Compare with (2). Competting electricity requires several successive transductions of energy.
4.	Compact and controllable effluent (waste).	Long electrical transmission lines.	Urban power and heat	Economics ultimately decides.
5.	Controllable energy pulses.	Chemical explosives, electromagnetic shocks.	Chemical processes? Metallurgical processes? Propulsion.	Reactor sources in principle repetitive without recharging. Range from BORAX to bombs unexplored. For nonrepetitive uses, see PLOWSHARE.
6.	Temperature limited by materials only.	Electricity, thermonuclear.	Chemistry and metallurgy. Radiant power (space).	Electricity indirect. Fission system directly accommodates to heat transport. Applicable to thermionic and thermoelectric generation of electricity.
7.	Direct source of ionization energy.		Chemical processes. Energy storage in solid state, or in chemical compounds. Pharmaceuticals?	Almost unexplored. Chemical intermediates may be used for electricity. Safanov's scheme is only "direct conversion" proposed that uses this principle.
8.	Source of various radiations.	Accelerators	Research isotopes. Industrial and biological irradiations.	Techniques worth exploring before applications are specified.
9.	Maneuverability	None	Propulsion. Peak load power and heat.	Conventional part of plant limits reinforces (2).
10.	Indefinite shelf life.	Some chemicals and fossil fuels.	Emergency power. Seasonal industrial and residential uses.	ALPR, APPR may be prototypes of such units; simple reactors expensive fuels. Reinforces (2) and (9).
11.	Transmutes elements and makes new heavy elements.		Makes new nuclear fuels. Special isotope production.	Breeders stretch nuclear fuel supply by factor of 100–500. New high isotopes have unexplored potentials.

[1] Born: Apr. 16, 1924; New York, N.Y. Parents: Abraham Spinrad and Rose (Sorrin) Spinrad. Brother: Leonard Spinrad. Married: Marion E(isen) Spinrad, 1951. Children: Alexander Abraham Spinrad, born 1952; Mark David Spinrad, born 1954; Jeremy Paul Spinrad, born 1956. Education: Public School 132, Manhattan, N.Y.; Columbia Grammar School, New York, N.Y.; Yale University; B.S. (with honors), December 1942; M.S., June 1944; Ph. D., June 1945 (physical chemistry). Post-doctoral research: Sterling fellow in chemistry, 1945–46 (Yale University). Employment: Clinton Laboratories (now Oak Ridge National Laboratory), 1946–48. Job title: Physicist. Argonne National Laboratory, 1949–present. Job title, 1949–54, associate physicist; 1954–56, senior physicist and head, Reactor Research Section; Reactor Engineering Division; 1956–57: Associate Director, Reactor Engineering Division; October 1957 to present, Director, Reactor Engineering Division.

Nature of employment: Reactor design, reactor physics, mathematics and theoretical physics, including:

(1) Design of MTR, CP–5, and Savannah River systems.

(2) Reactor physics analysis on above reactors and supervision and evaluation of data from reactive experiments such as: critical experiments for MTR, exponential and critical experiments for Savannah River, exponential experiments on fast reactors, multigroup methods; and transport theory.

(3) The Monte-Carlo method, and the programing of computing machines.

(4) Internal conversion of electrons, and nuclear cross sections.

Organizations: Sigma Xi (Yale chapter); RESA (Argonne branch); American Physical Society (fellow); American Nuclear Society (charter member); Gamma Alpha Graduate Scientific Fraternity (senior member, Yale and Chicago chapters).

Committees, etc.: AEC Computer Council, 1953–55. AEC Reactor Physics Planning Group, 1951–present. American Nuclear Society, past chairman, Chicago section. AEC, Military Reactor Classification Subcommittee. AEC, Criteria Committee on Reactor Safety.

Present address: 845 Wellner Road, Naperville, Ill.

Dr. SPINRAD. In surveying advanced reactor concepts, I have chosen to classify nuclear systems according to the features of fission energy which they illustrate. I believe that this, rather than classification by reactor type or application, will enable us to perceive the ultimate goal of nuclear energy somewhat more clearly.

In the accompanying chart I have listed some 11 characteristics of nuclear fuels or of reactors which are in some way unique. That is to say, these are the characteristics by which nuclear reactors can perform some function cheaper, better, or more efficiently than known alternates. Few of the features in the chart are novel. Some of them are indeed the subject of intense development right now. Some of the suggested concepts are covered in other sessions of these hearings.

The first item on the chart provides the impetus for our current development efforts on central station power and on low quality process heat. I include it because I would like to emphasize that the intrinsic low cost of the fuel as mined represents the primary advantage of nuclear reactors in this application. I believe that the current tendency to evaluate power reactors by reactor category rather than fuel category obscures this fundamental fact. From this observation, I conclude that power reactor technology ought to be bent toward using the nuclear fuel in the most economical fuel cycle. Capital cost of reactors can be reduced, but there is no particular virtue to nuclear power unless the intrinsic low cost of the fuel is preserved.

The second item to which I call your attention is the unique compactness of nuclear fuel. One gram of U^{235} is capable of producing about 24,000 kilowatt-hours of heat. When this capability is coupled with the intrinsically compact structure of nuclear reactors, all sorts of applications where fuel or system size and weight are restricted may be suggested. Nuclear powerplants such as the Army package power reactor and Argonne low power reactor have been developed for use in remote locations, and such systems can enormously improve the capabilities of permanent Antarctic stations. Logistics also enters into the use of reactors for maritime and other propulsion. The most exciting extrapolation of this feature to me is the possibility of using reactors not only for power sources in satellite stations and for electrical propulsion of space ships, but, indeed, for setting up large permanent bases on the Moon and Mars. With enough energy one needs to transport only shop and laboratory equipment to such locations. Ceramics and metals can be manufactured from local rock and converted into the materials for construction of living quarters, complex machines, and, in fact, just about everything but biological necessities and (perhaps) the fuel itself. The systems to do this must have very high power ratings; be compact for these power ratings; be operable at such high temperatures that radiation or heat is feasible; and be extremely rugged and reliable under specific design conditions which might range from several g. accelerations to almost weightless conditions.

My opinion is that such reactors can indeed be developed. The development, it is true, would be lengthy, but there is no intrinsic obstacle that I can foresee. I believe that various adaptations of fast reactors are the most promising systems for this application. The reasons are that fast reactors are intrinsically assemblies of fuel and

coolant only; that they have already started development in the direction of high temperature with sodium and other liquid metal coolants; and that they are well adapted to compact processes for repurifying and reconstituting spent fuel. Moreover, they can make good use of auxiliary components as neutron reflectors.

The next item on my chart refers to the fact that nuclear fuel does not need any other material for its burning. Most common chemical fuels assume the presence of oxygen in some form, and, in fact, the oxygen used in burning coal or oil weighs more than the fuel itself. This aspect of nuclear energy reinforces the desirability of its application in space. It also suggests use in such airless environments as the bottom of the ocean. I believe that Dr. Hall will have something to say on the application of this principle. I must remark that a reactor using sea water as a coolant, which would appear at first blush to be the most logical type of system, presents to me such formidable technical problems that I must postulate a number of technical breakthroughs to accomplish the goal. Particular problems are coolant activation and the extreme corrosiveness of sea water.

The same feature makes nuclear heat chemically passive and suggests the possibility of its use for chemicals and metallurgical processes where high temperatures are desired.

The next item, the controllability of nuclear wastes, is a bit of reverse English. Rather than considering fission products as a danger, I personally believe that the degree to which we have been able to contain these products is a basic advantage. In the long run, I think even increased carbon dioxide from fossil energy sources is perhaps a more disturbing factor in our environment. For this reason, I expect that ultimately nuclear power will be particularly preferred for large urban centers.

Mr. RAMEY. Isn't that one of the reasons that Commonwealth Edison of Chicago is interested in their Dresden plant and other plants in the Chicago area, because of the sulfur problem?

Dr. SPINRAD. The effluent problem is serious as far as I know in some places. I don't think it is particularly important in the Chicago area yet. But as one postulates the further growth of larger and larger cities, you do ultimately reach a point where the cities get very large and the pollution of the air is extremely difficult to control.

One feature of nuclear energy which has not been very much explored is its ability to deliver sharp and controllable pulses of energy. The Plowshare Symposium has already illustrated some applications of the largest and most violent pulses. However, except as sources of neutrons for research purposes, not much consideration has been given to technical applications of pulsed systems. We already know how to build pulsed reactors which can, as Argonne's Treat reactor, repetitively deliver pulses of 10,000 megawatt seconds within a tenth of a second. The Los Alamos Godiva system can shorten the time scale of the pulses by another factor of 100. It is, in principle, possible to convert this energy directly into mechanical shocks. These are traveling pulses of very high temperature and pressure. The best analogy might be a nuclear machinegun. Such a gun might be useful for pulse jet propulsion systems, or it might be possible to take advantage of the high transient temperatures and pressures for selected chemical processes.

High temperature nuclear heat can be furnished at temperatures beyond those available from chemical fuels. Nuclear heat is temperature limited only to the extent that we can retain materials in solid or liquid form, whereas, chemical heat has other intrinsic limits. This capability of nuclear systems has already received some attention for propulsion as in the Pluto and Rover systems. The same capabilities are quite intriguing for chemical and metallurgical processes. The Turret system at Los Alamos is a "first look" at this type of system and I believe that Drs. Manowitz and Perry will both have remarks on nuclear heat.

The Turret system is limited in temperature potential by the fact that heat is generated in a solid and transferred into a process gas. The temperature limitations may be further relaxed by going to systems in which the nuclear fuel is suspended in and circulated with the process gas. Problems of radioactivity are quite akin to those which will be pointed out by Dr. Manowitz in connection with use of ionizing radiation.

A "wild blue yonder" suggestion is that for space application an efficient reactor can be made into an artificial sun radiating heat directly to the working location. Another application connected with the high temperature potential of nuclear heat is based on the assumption that thermionic and thermoelectric conversion schemes become highly efficient above flame temperatures. If so, these schemes may have unique application in nuclear systems. They will be discussed further in tomorrow's session.

The next item is perhaps the most unique feature of nuclear energy. Virtually all of the energy of the fission process goes into ionizing materials in the reactor, and heat appears only when these ions are recombined. Dr. Manowitz will cover this aspect in certain chemical applications. I can add some supplementary remarks to the effect that recent advances in chemical fuel cell technology make it possible to use almost any chemical reaction product which indeed stores energy. Energy may also be stored in solid state devices, although this use might not be highly practical. Finally, there is one scheme, that of Dr. George Safanov at Rand, which makes use of the fact that the fission fragments are ions which can perform useful work against an electrical field and thereby generate electricity directly.

A reactor as a source of a variety of radiations is a well-known research tool. In this sense a number of unique applications have already been developed. I would like to take the liberty here of departing slightly from the main subject of this presentation to argue for a greater emphasis on the use of reactors as research tools for two reasons; first, and most obviously, the research itself can lead us as it has in the past in extremely useful and fruitful directions; second, the history of reactor development indicates that an overwhelming number of technological advances in other applications are pioneered by applications in research reactors. For example, the concept of the *Nautilus* developed out of the materials testing reactor, and the Savannah River reactors borrowed a good deal from CP 3 and NRX experience. In brief, when a research man asks for a crazy specification and it can be provided, the chances are very high that sooner or later an industrial application will pop up. In view of this, a more

active general program in development of reactors as research devices is logical even where specific research needs are not yet justified.

The next two items, maneuverability (or ability to change power rapidly) and indefinite shelf life, are quite closely related. The power of a nuclear reactor can be conveniently regulated over factors of at least a quadrillion with considerable ease. Indeed, limitations on this maneuverability are found primarily in the auxiliaries of the system. An extreme case is the use of a reactor as a largely off system for use under seasonal or peak-load conditions. These characteristics make such systems as ALPR and APPR particularly useful for disaster relief and a good number might be stocked against such needs. Although local economic factors are governing, it is also not out of the question that nuclear power can be used as a seasonal supplement to hydropower, in such seasonal industries as lumbering, or for peak-load central station electricity. In these latter applications the cost of intermittent fuel supply is often so high that high capital cost nuclear systems may indeed be tolerable.

The final unique characteristic of nuclear energy represents its oldest use; namely, to transmute elements, particularly the heavier ones, by virtue of the intense neutron bombardment of reactor materials. We are used to the availability of standard isotopes from tritium to plutonium. We are just beginning to explore isotopes of elements beyond plutonium on the periodic table. The Russians are building a system to make such isotopes, and we are planning to surpass their stated goals with the Oak Ridge high flux isotope reactor. A backup for this target exists in the planned capability of the Argonne high flux reactor which has a primary research function. Among the transuranic elements of greatest purity, I would like to single out Californium, element 98, and, particularly, its mass 252 isotope. This isotope undergoes spontaneous fission with a half life of about 2 years and if any quantity of it could be made, it would represent by far the most concentrated power source available to us. I might add that the ability to furnish reactors for this production is another example of an application arising from study of research systems.

In summary, nuclear power needs more intense exploration, not only for large, baseload, central station plants, but also for remote—including space and undersea—seasonal, peak load and emergency uses. Nuclear heat must be explored more fully, particularly for high temperature metallurgical process application. The use of reactors as pulsed sources of energy or as ionizing agents requires a new series of researches, with the most probable payoff being in the chemical industry. The exploration of new systems for research must be encouraged for its own sake, as pointing the way to other unknown applications.

Representative PRICE. Doctor, with reference to the possible eventual construction of a reactor on the Moon and Mars, were you suggesting that we do not now have the blueprints of the type of reactor that would be suited up there, and we have to look for something entirely different than we have?

Dr. SPINRAD. I am not completely privy to the innermost councils in this matter, if such exists, indeed. But from what I have been able to gather, the use of nuclear energy in space has been limited

in its planned direction to relatively small systems, whereas what I am talking about are systems of extremely large capability, one could say thousands or more megawatts of capability, systems which are equivalent to supplying quite a large city. I think that is what is needed to get this sort of permanent station going. These are very, very different, I think, from the sort of systems that are used to generate power for transmission of information obtained from instruments.

Representative PRICE. On the same page with respect to the application of reactor technology in space, you discuss the exploration of nuclear fuel. Do you think that we are exploring nuclear fuel in the AEC fuel cycle program for a more economical fuel cycle?

Dr. SPINRAD. I think that this type of fuel cycle program has a good deal of merit. I suppose I could say I was taking a poke at something in this particular remark, and I might as well say what it is. That is a tendency within the nuclear industry to consider reactors by type, PWR, BWR, organic, and so on. It seems to me that the intrinsic differences in ultimate usefulness are far smaller as between any pair of those that has the same conversion ratio and uses fuel of the same enrichment than the differences. The characteristics of the fuel cycle, to me, govern the reactor concept rather than the characteristics of the coolant, moderator, and so on.

Representative PRICE. Mr. Hosmer, do you have any questions?

Representative HOSMER. No; but I would like to go down this list of 11 features. I think we have here something with probably a few minor drawbacks.

Dr. SPINRAD. A few minor drawbacks, some of which we don't know what to do about.

Representative VAN ZANDT. I was impressed by your statement on page 4 where you say in the long run "I think increased carbon dioxide from fossil fuel is probably a more disturbing factor in our environment than waste material," and so on. Does that mean that the smogs that we are confronted with offer more danger to the human being than the radiation from the waste material? Is that what you have in mind?

Dr. SPINRAD. That is one man's opinion; yes, sir.

Representative VAN ZANDT. Some years ago we had a question put to us by someone from Los Angeles who was concerned about smog. He traced much of it to automobiles, and to eliminate it suggested the utilization of atomic energy in automobiles. Do you have something in mind along these lines?

Dr. SPINRAD. I wish I could be optimistic about that. As we all know, nuclear shields are pretty heavy in order to make radiation less dangerous. The only way I could conceive of nuclear energy to run automobiles is to generate electricity which is used to charge up storage batteries and go in for electric engines.

Representative VAN ZANDT. Is there a possibility that radiation from a nuclear engine in an automobile could be less hazardous than the smog in Los Angeles?

Dr. SPINRAD. There are two points of view——

Representative HOSMER. Would the gentleman mind making the example San Francisco?

Dr. SPINRAD. There are two points. First, that the radiation from a properly performing reactor is indeed quite negligible, well below medical background and so on. Second, my statement is a statement of faith in our ability to keep these wastes bottled up without paying an arm and a leg in costs for it. If you can do that, you indeed have no dangerous effluent.

Representative VAN ZANDT. Will you go back to the statement you made a moment ago when I think you mentioned the fact that we should use the energy produced from uranium for the purpose of charging batteries? Did I understand you correctly?

Dr. SPINRAD. Yes.

Representative VAN ZANDT. To be used in automobiles?

Dr. SPINRAD. Yes. Electricity is a pretty ubiquitous form of energy. It can be used to run all sorts of motors including automotive motors.

Representative HOSMER. Wouldn't direct conversion be the more direct approach? I mean the direct conversion to electricity?

Dr. SPINRAD. Direct conversion of nuclear or chemical?

Representative HOSMER. Thermoelectrical conversion.

Dr. SPINRAD. Thermoelectrical conversion is not really direct conversion. It is just a substitute for an alternating motor.

Representative HOSMER. It gets rid of a lot of moving parts and perhaps permits the condensation of size to the point where you could use a radiation heat source and so forth.

Dr. SPINRAD. You still have the problem of shielding regardless. This does not get around that. Whether you like it or not—we would like to try to make use of the fact—there is an awful lot of radiation floating around and the smallest shields for the smallest powers are still several feet thick.

Representative HOSMER. Doesn't this field of direct conversion offer some rather attractive possibilities?

Dr. SPINRAD. Yes, indeed it does. I think you have put your finger on it already, sir. The elimination of moving parts and of at least one type of complex mechanical equipment in converting energy is always a very useful thing. There is an equivalent sort of potential in what are called the fuel cells for the chemical field where you can hope to burn chemical fuels and oxygen without any flames at all, with electricity as the direct product.

Representative HOSMER. Do you know any work that is being done to tie this to a nuclear reactor?

Dr. SPINRAD. No work right now. We have been making some plans and hopes at Argonne to tie in some electrochemical research into our program.

Mr. RAMEY. The plasma thermocouple is the type, if you could use a reactor fuel element as your source of heat.

Dr. SPINRAD. Yes.

Mr. RAMEY. I think that will be discussed Friday morning.

Dr. SPINRAD. I suspect there will be quite a bit more to say about it. I know the witness knows more about it than I do. The only remark I have to make is that again while this is a very attractive ingenious and exciting development, it really should not be called direct conversion. It is really a use of nuclear heat. Because the energy of the

fission is first converted into heat within the reactor and then converted to electricity by this process.

Representative VAN ZANDT. On page 5 of your statement you take us out into the wild blue yonder by suggesting that for space application an efficient reactor can be made into an artificial sun radiating heat directly to the working location. Would you amplify that thinking a bit?

Dr. SPINRAD. Very well. This is a suggestion that one of the folks at our laboratory came up with a little while ago. I don't know quite what to do with it except to throw it on the table, or up in the air some place. The point is that a hot body generating power is generally under some compulsion to expand. However, as long as it retains a solid form it will stay in one piece. This expansion and staying in one piece represents a degree of control so that the temperature can be regulated about the criticality conditions of a reactor. If one has a gravitation-free environment, then one can also tolerate the liquid phase. You will just get a big ball and it will glow. The size of the ball will be governed by the mass of uranium in it. Some of the more refractory uranium compounds stay liquid up to 3,000° or 6,000° K. or thereabouts, which is the temperature of the surface of the sun. So it is not inconceivable, although it may never be practical, to throw a big ball of uranium oxide, let it start glowing, and when you add the last gram the temperature goes up 6,000° and stays there, radiating its heat. It would be a small body, perhaps several feet across.

Representative VAN ZANDT. You certainly would have something to do with the climate in various parts of the world.

Dr. SPINRAD. I would not think of that as a climate control. I would think of this more or less as a space application. Perhaps if one can afford to be quite wasteful, as I suspect we could, to allow the folks at Antarctica to take sun baths of a sort, if they used lead glass.

Representative PRICE. Thank you very much, Dr. Spinrad. Since you are going to be on the panel in a few minutes, why don't you move over there and remain at the table.

The next witness this morning is Dr. David B. Hall of Los Alamos Laboratory. Dr. Hall, will you proceed?

STATEMENT OF DAVID B. HALL,[1] LOS ALAMOS SCIENTIFIC LABORATORY

Dr. HALL. Thank you, gentlemen. I am very happy to be able to support the testimony which you have just heard from Dr. Spinrad, although there was no specific collaboration beforehand. I had the opportunity of seeing a copy of his remarks a few days ago, and I was pleasantly surprised to find that many of the things which he was saying were ones which I had intended to cover in my remarks. This is really not an embarrassment; it is indicative of the fact that those

[1] Born: East Orange, N.J., Jan. 2, 1915.
Married: 1939, two children.
Education: B.S., Rutgers University (1937); M.S., University of Chicago (1939); Ph. D., University of Chicago (1942).
Positions, past and present: Instructor of physics, University of Denver, 1942–43; Manhattan project, Chicago, 1943–45; staff member, Los Alamos Scientific Laboratory, 1945 to present.
Societies: Fellow, American Physics Society; member, American Nuclear Society.
Specialties: Cosmic ray physics, neutron physics, nuclear reactor development.

of us in the laboratory working with fuels are continually searching for the natural role of atomic energy in the family of fuel sources.

Of the topics which Dr. Spinrad has listed as being unique, I would choose to emphasize four as being particularly important in determining the position of nuclear energy derived from power reactors in the complex of energy sources. These are first, the low cost of fuel as it is mined. I think the emphasis should be on the fact that it is as it is mined, not as it is fabricated.

Secondly, the compactness of the fuel or the concentrated source of the energy.

The third item would be the lack of oxidant being required for release of the energy.

The fourth item would be the very high temperature limitation or lack of temperature limitation.

The application of reactors which has been emphasized by all of the countries to date is that of developing electrical power generation. This presents a very clear objective and a useful starting point in adapting nuclear power for peaceful purposes. But the generation of electrical power is not the major consumer of our fuel resources, as has been pointed out in many surveys. Using figures for the year of 1956 as compiled in the AEC quarterly, the Power Reactor Technology, less than one-fifth of the energy consumed in the United States today is used for electrical energy generation. Process heating and space heat account for about one-half. In the present stage of our national economy, electrical energy generation amounts to about 700 billion kilowatt-hours annually, and, at an average generating cost of 6 to 7 mills per kilowatt-hour, is approximately 1 percent of our gross national product.

The cost of electrical power is obviously very vital to an important segment of our industry but it represents a relatively minor position in the overall economy. This is not to say that a significant change in the cost structure of electrical power would not be reflected in changes in the importance of a few products, even many products, and might even result in painful dislocation of some selected areas. At the same time, it is difficult to see how the national economy can be affected in a significant manner by replacing centralized electrical power generating stations, using conventional fuels, with ones using nuclear energy. I think these statements should be interpreted as pointing out that the power industry has done its job so well that there appears to be little margin for improvement and makes the work of developing nuclear power just that much harder. But if the cost of the generation of electrical energy went to zero, the resulting cost of fabricated items in the market could not change very greatly. One would still have charges for plant, labor, distribution, taxes, and so forth; all of which represent more than 90 percent of the value of the fabricated items.

A survey of typical fabricated goods will show that the cost of energy contributes about 3 percent to the product value, and only rarely is it more than 5 percent. The cost of electricity in the home for the average small consumer is considerably greater than the bare generating cost.

However, in the not too distant future there may be compelling reasons to require that sizable blocks of fuel consumption be required to utilize nuclear energy in place of conventional fossil fuel. It might,

for example, be motivated by the need to conserve natural fluid fuels such as oils and hydrocarbons for mobile power uses. Or alternatively, the increases of carbon dioxide content in the atmosphere from ever-increasing consumption of fossil fuels might conceivably be altering the temperature and hence the sea level by affecting the cloud formation and rainfall. There is no direct evidence for this now. Both of these effects would tend to make the average climate warmer and drier. In this connection one can point out that there are bodies of water in this country which have disappeared only recently after having been in existence for many thousands of years, and supporting marine life over that period. This is possible evidence that man is changing his environment.

The same intrinsic feature of a nuclear powerplant that makes it so attractive for a space power station, that is, requiring no oxidant, also suggests extensive applications in the submarine environment. These have been explored in a survey conducted by the Stanford Research Institute in Report SRIA–7, dated August 1959, and several ideas appear to have merit. Among them is the application to oceanographic research where the requirement would be for a small powerplant output with a very long fuel life and a vessel capable of withstanding the required pressures. Presently available equipment is severely hampered by the lack of mobility and of power for undersea exploration. These may not appear to have commercial value now but authorities working in oceanography have stated that even now we know less about the oceans than we do about outer space. It seems almost inescapable to me that if the day comes when the population of the world cannot be adequately supported by land grown foods we will have better success in augmenting food supplies by farming the seas than by cultivating the planets.

Another important resource of the seas lies in the mineral content of the oceans both as dissolved salts and as precipitates in the sediments. The use of nuclear explosives might be feasible in breaking up a mineral belt on the ocean floor and preparing the ore for recovery by a deep sea dredge. Hydraulic lift or jet pumps working on the sea bottom might utilize nuclear energy efficiently in a marine environment. Important and critical minerals in this way may become available to nations now dependent on importation. These considerations are a matter of detailed economic evaluation.

The prospect of using reactor power for process heat, using the term very loosely as a way of differentiating between electrical energy generation and nonelectrical energy stationary power, has not received all of the attention which it may merit in the long-range picture. This statement is based on the conviction that the valuable fuels which support our highly mobile way of life and our flexible marketing techniques are being depleted much more rapidly than the total reserves increase; two-thirds of our present energy is obtained from oil and gas, with a consumption rate of 0.04 Q per year and an indicated reserve of one to two Q. A Q you may recall is 10^{18} B.t.u., a very large number. If this pattern continues or worsens, we will have to find a replacement fuel or change our way of living. Nuclear energy itself is not suitable for this application as has been mentioned, nor is coal, but the energy obtained from nuclear reactors can be used for the synthesis of hydrocarbon fuels and for the manufacturing of basic materials to be used in fuel cells.

Speculations of this type which try to probe the future are at best difficult to substantiate, and in no case have I tried to make an attempt to establish the feasibility, either technically or economically, of any of these suggestions. They are mentioned here only as statements of faith, that the potential of the role of nuclear energy is far broader than that of generating electrical energy by conventional steam turbine equipment. It can be stated categorically, however, that the more efficiently and more cheaply the utilization of power from fission energy is made, the more widespread its application will be. The property of a potentially cheap energy source may well be the outstanding characteristic of nuclear power. In order to exploit this feature, it will be necessary to depart radically from what have become standard concepts of complicated fuel elements, and the costly refining and fabrication stages which nuclear fuel must undergo and the attendant auxiliary costs of precise positioning and handling of these fuel elements. These requirements in present reactor concepts do much to nullify the low cost of fuel as it is mined.

Representative PRICE. Thank you very much, Dr. Hall.

(Dr. Hall's formal statement follows:)

SOME SPECIAL APPLICATIONS FOR NUCLEAR REACTORS

The energy obtained from nuclear reactors has several unique properties when compared to other forms of fuel. Consideration of these special features can lead to speculation concerning applications where atomic energy might be used in the future to particular advantage. Some of these applications are under development now as part of the AEC long-range program. Others are logical outgrowths of current or planned development, and yet others are merely fragmentary possibilities.

Some of the unique properties which can be considered are the concentrated nature of the energy source, the limitless temperature of the energy, the self-contained feature which requires no oxidant to be present, and the relatively low cost of the fuel before fabrication. Some special applications which take advantage of these unique features may be found in regions outside of the earth's atmosphere, either in far outer space as satellite power stations, or under the sea as special submarine vessels for exploration, or mining operations. If the inexpensive nature of the fuel can be fully exploited, it is conceivable that changes in climate can be effected over a modest area at reasonable costs. Less than one-fifth of the energy consumed in the United States is used for electrical generation; process heat and space heat account for about one-half. At the present stage of our national economy, electrical energy generation amounts to approximately 700 billion kilowatt-hours per year and, at an average generating cost of 6 to 7 mills per kilowatt-hour, represents only 1 percent of the gross national product. The cost of electrical power production is obviously vital to an important and vocal segment of our industry, but it represents a relatively minor position in overall economy. This is not to say that a significant change in cost structure of electrical power would not be reflected in changes in the importance of a few products and might even result in painful dislocations in some selected areas of our national life. At the same time it is difficult to see how the national economy can be affected in a significant manner by replacing centralized electrical power generating stations using conventional fuels with ones using nuclear energy.

However, in the not-too-distant future there may in fact be compelling reasons to require, through legislation, that sizable blocks of fuel consumption be required to utilize nuclear energy fuel in place of conventional fossil fuel. This might, for example, be motivated by a need to conserve natural fluid fuels such as oil and hydrocarbons for mobile power uses. Alternatively, the increases in the CO_2 content of atmosphere from ever-increasing consumption of fossil fuel might conceivably be altering the climate and geography of the world both through increasing the mean temperature and hence the sea level and also by affecting the cloud formation and rainfall. Both of these effects would tend to make the average climates warmer and drier. It is a matter of record that bodies of water have

recently disappeared after supporting marine life for many thousands of years, a fact which gives credence to the theory that man is changing his environment.

In the Los Angeles area of the California coast, a temperature inversion pattern in the air is apparently a normal phenomenon for long periods of time. This is a departure from the situation which prevailed in the recent past as little as 30 to 50 years ago. Again, it is possible that this changed weather condition and its influence on the local climate is the result of the fuel burning in the congested metropolitan area. It has been suggested that the temperature inversion pattern could be upset over a limited region by operation of a nuclear powerplant in the offshore waters. Such a reactor would have to be very large to heat an air mass sufficiently to literally punch a hole in the warm barrier layer at the inversion point. However, this same large reactor might be distilling sizable amounts of sea water to support the area. Estimates have shown that a station of 100,000 megawatts (7 percent of U.S. fuel consumption in 1956) operating at relatively low temperatures could produce these results. Extreme as this suggestion is, it would be absurd with any other fuel.

The same intrinsic feature of nuclear powerplants that make it attractive for space power stations, i.e., no requirement of an oxygen source to liberate the energy, also suggests extensive applications in the submarine environment. These have been explored in a survey conducted by the Stanford Research Institute (SRIA–7, dated August 1959) and several ideas appear to have merit. Among them is the application to oceanographic research where the requirement would be for a small powerplant of modest output with very long fuel life in a pressure vessel capable of withstanding the required pressures. Presently available equipment is severely hampered by lack of mobility and of power for undersea exploration. This may not appear to have commercial value, but authorities working in oceanography have stated that even now, we know less about the oceans than about outer space. It seems almost inescapable that if the day comes when the population of the world cannot be supported by land-grown foods, we will have better success in augmenting the food supplies by farming the seas than by cultivating the planets. Another important resource of the sea lies in the mineral content both as dissolved salts and as precipitates in the sediments. The use of nuclear explosives (Plowshare) might be feasible in breaking up a mineral belt on the ocean floor and preparing the ore for recovery by a deep-sea dredge. Hydraulic lift or jet pumps working on the sea bottom as dredges might utilize nuclear energy efficiently in a marine environment.

Speculations of this type which attempt to probe the future are at best difficult to substantiate, and in no case is there an attempt here to establish the feasibility, either technical or economical of the suggestions which are made. They are mentioned only as statements of faith that the potential role of nuclear energy is far broader than that of generating electrical energy by conventional stream turbine equipment. As we learn how to utilize this energy source, new applications will suggest themselves to us as outgrowths of the work which is underway.

Representative PRICE. The next witness will be Mr. Bernard Manowitz of the Brookhaven National Laboratory.

STATEMENT OF BERNARD MANOWITZ,[1] BROOKHAVEN NATIONAL LABORATORY

Mr. MANOWITZ. Thank you, Mr. Chairman.

The subject matter that I am about to treat is the use of nuclear reactors for the production of industrial chemicals.

[1] Born in 1922 in Jersey City, N.J. He has an M.S. degree in chemical engineering from Columbia University. From 1944 to 1947 he worked at Clinton Laboratories, now Oak Ridge National Laboratory, primarily in the field of pile engineering. From 1947 to the present he has been at Brookhaven National Laboratory. There his responsibilities have included the design and construction of the Brookhaven waste disposal system, engineering research on waste processing methods, fission product utilization studies, and research and development work in radiation physics and radiation chemistry. He is now the Associate Head of the Chemistry and Chemical Engineering Division at Brookhaven. From 1952 to 1954 he was a member of the AEC Waste Processing Committee. He was also an adviser to the delegation at the 1955 Geneva Conference where he presented a U.S. paper on waste disposal, and was also a delegate to the 1959 Warsaw conference on large-scale radiation sources. Mr. Manowitz is author of a considerable number of publications both in the field of waste disposal and in the field of applied radiation. At the present time he is also an editor of the International Journal of Applied Radiation and Isotopes.

It is possible that nuclear reactors can be used for producing industrial chemicals. However, the technological problems involved are considerable, and there are many alternate ways of making the same chemicals, both today and in the far future. Therefore one should approach this problem with a certain amount of cautious reserve.

Nevertheless, the problem has a great amount of scientific interest, and the determination of the feasibility of this venture and the definition of the problems one faces constitute a worthwhile scientific effort.

We have been studying the use of intense radiation fields for initiating chemical reactions for quite some time at Brookhaven, and have recently concluded two studies for the AEC on the feasibility of using nuclear reactors of various designs for the industrial scale production of fixed nitrogen. I am happy to have this opportunity to tell you something of these studies and of their results.

If you recall, the fission process involves the absorption of a neutron in uranium 235 resulting in the production of more neutrons with a good deal of kinetic energy, and of two fission fragments, also with a good deal of kinetic energy. If this energy is dissipated into heat, we get the familiar result that fissioning uranium is a very concentrated heat source. However, it is possible to use some of the kinetic energy, not only of the neutrons and fission fragments, but also of the beta rays and gamma rays that ensue, to break chemical bonds and form active species, which, in recombining, will form new chemical substances. We are thus using the radiation power of the reactor rather than the heat power.

The distribution of the kinetic energy of fission is such that the fission fragments carry 85 percent of the energy, the neutrons carry 2 percent, and the beta rays and gamma rays carry most of the rest. The fission fragments, the fission neutrons, the gamma rays, and the beta rays, all have ionization power, and it is possible to design reactor systems that make use of any or all of these radiations. The characteristics of the reactor and of the side products and side reactions that can be produced will vary greatly from case to case. However, you will note that 85 percent of the total energy of fission is in the kinetic energy of the fission fragments. That is as this atom of uranium explodes, these two halves carry most of the energy with them.

If one desires to make use of the energy of the fission fragments, one must design a system in which an appreciable portion of the energy of the fission fragments is absorbed in the desired medium. Fission fragments have ranges in the order of microns in solids, millimeters in liquids, and in the order of centimeters in gases at atmospheric pressure. Therefore, if solid fuels are to be used, they must have dimensions in the order of microns if the fission fragments are to escape. Thus we are faced with a problem of new fuel technology, and one in which it will be more difficult to produce a satisfactory fuel than it has been in the power program.

Futhermore, the use of the fission fragments contaminates the chemical product with fission products, and it will be necessary to decontaminate the chemical product before it can be marketed.

Because we are dealing with large quantities of energy when considering fission fragment nuclear reactors, the chemicals to be pro-

duced should have a potentially large tonnage market. Let us proceed then to consider some of the promises these reactors may have before reconsidering some of the problems.

Although the general subject of radiation chemistry has been studied for many years, there is a surprising lack of data on the yields of many chemical reactions that one might like to consider in connection with the use of nuclear reactors. By far the most data available is on nitrogen-oxygen reactions. Fortunately, this is probably the reaction having the most national interest. Briefly, if one exposes nitrogen and oxygen—the constituents of air—to fields of ionizing radiation, one fixes the nitrogen, that is, the oxygen and nitrogen combine to form a new chemical compound, nitrogen dioxide (NO_2). As I say, some data are available on the yields of nitrogen dioxide formed when oxygen and nitrogen are exposed to fission fragment radiation. When this chemical compound reacts with water, one produces nitric acid, an important commodity. When this chemical compound reacts with ammonia, one produces ammonium nitrate, a very important fertilizer.

Thus, as I will show in more detail later, it is possible to design a special nuclear reactor, a chemo-nuclear reactor as we call it, and an associated chemical complex which uses as raw materials only air, water, and uranium, and produces either—

(1) a combination of electrical power and nitric acid;
(2) a combination of process steam and nitric acid; or
(3) fertilizer with no associated power or process steam.

To return to the problems of designing a chemonuclear plant, it turns out that there is a complex relationship between the nature of the raw materials, the radiation chemistry of the reaction, the physics of the nuclear design, and the purification of the chemical product, and that each of these parameters fixes a design condition for the other. The nitrogen fixation system is a good illustration of this point.

The raw material, for example, cannot simply be atmospheric air. Air contains sufficient argon so that in any one-pass system the argon–41 produced by neutron absorption would be so great that atmospheric pollution would preclude operation of the reactor. It is necessary, therefore, to separate argon in an air liquefication plant. This argon is probably marketable. The use of the air liquefication plant permits one to adjust the composition and flow rate of the feed to equal the stoichiometry and production rate of the products withdrawn, thus permitting one to recycle the gas stream until a high concentration of product is built up.

An important design parameter is the required mass ratio of fuel to gas. In order to deposit as much fission fragment energy as possible in the gas, the gas should be at as high a pressure as practical. In order that power be extracted from the reactor, the gas should be at as high a temperature as practical. The radiation chemistry of the system at these temperatures and pressures dictates the retention time in the reactor, and the ratio of fuel and moderator to gas.

Three types of fuel have been considered for such a chemonuclear reactor. One is a glass wool containing a high concentration of uranium oxide dissolved in the glass. Because the filaments are very thin a large fraction of the fission fragments escape into the gas

(although an appreciable amount is recaptured in the glass filament). Another is an array of flat ribbons on which a very thin coating of uranium oxide has been plated. The ribbons are placed sufficiently far away from each other so that no fission fragments reenter the next ribbon. By making the ribbons sufficiently thin and supporting them on a grid, the efficiency of the ordered array becomes comparable with the glass wool case. A third type of fuel is a dispersion of very fine uranium oxide particles in a circulating gas stream.

The circulating dust system has the disadvantage of being an inherently unstable system with questionable nuclear stability and with potential problems in erosion of container materials, caking, hot-spots, fuel recovery, and gas circulating equipment.

The fixed fuel systems require somewhat less technological development, but have serious potential problems in nuclear safety. These systems inherently have positive temperature coefficients coupled with large excess reactivities and relatively small increases in temperature or decreases in pressure might put the reactor into a dangerous prompt-critical condition.

All of these systems have the problem of decontaminating the chemical products from the volatile and nonvolatile fission products, and from the radioactive corrosion products that they will contain. Fortunately, the nitrogen and oxygen will not themselves be radioactive, and it should be possible by suitable physical and chemical techniques to separate out the radioactive species.

I wish to emphasize that although I have mentioned all of these difficulties to indicate the complexity of the system, I believe all of these problems to be soluble, and feel there is no inherent technological barrier to the use of fission fragments in the production of industrial chemicals. Indeed, we have made fairly detailed designs of all three reactor systems and associated chemical plants, on which cost estimates have been made which we think are probably achievable.

The power level of the reactor varies directly as the production rate required, i.e., the tons per day of fixed nitrogen produced, and inversely to the chemical yield of the desired product and to the efficiency of energy deposition in the gas stream. We have considered two cases—one involving a fixed nitrogen production rate of 200 tons per day in terms of nitrogen dioxide, the size of the small plant built today—and the other about 1,200 tons per day, the size of a large plant. These require reactors of about 150 thermal megawatts and 700 thermal megawatts, respectively.

The results of our economic analysis are as follows:

1. At the present state of our knowledge there is not much difference in cost of the reactors using the different types of fuels described previously.

2. None of these chemonuclear systems are competitive with present-day fixed nitrogen plants using fossil fuel at 40 cents per million B.t.u.'s.

One system considered was a plant that produced power and nitric acid and sold the power at 7 mills per kilowatt-hour. For the smaller scale plant (200 tons per day) the manufacturing cost appears to be about two or three times as expensive, and at the larger scale (1,200 tons per day) about 20 to 40 percent more expensive.

Another system considered was a plant that produced steam suitable for water desalination and nitric acid and took 50 cents per million B.t.u. credit for the steam. This was a large plant producing 1,200 tons per day of nitrogen dioxide and able to distill 60 million gallons per day of water. The fixed nitrogen turned out to be about 30 percent more expensive than at today's costs.

In comparing chemonuclear reactors with other methods of making fixed nitrogen, consideration must be given to the use of conventional nuclear powerplants. Electrical power can be used to produce hydrogen from water by electrolysis. This hydrogen can be converted to ammonia by a standard catalytic process. The ammonia in turn can be oxidized to nitric acid, which is the conventional way of making nitric acid. Our study indicated that a conventional nuclear powerplant coupled to an electrolytic hydrogen and ammonia plant would produce nitric acid at about twice the manufacturing cost of today's processes.

One system that looked very interesting to us was a dual cycle system (chart 1). One would use the power produced by the reactor to produce hydrogen and then ammonia, and at the same time produce nitrogen dioxide from the fission fragments. The ammonia and nitrogen dioxide would then be reacted to form ammonium nitrate. A balanced plant can be designed that would not have any excess power to sell, but would convert air, water, and uranium directly to fertilizer. On a large scale we estimate this plant would produce fertilizer at costs about 30 to 40 percent greater than in areas where fossil fuel is available at 40 cents per million B.t.u.

At this point it is probably worth while to consider the present and potential market for fixed nitrogen. The present world production of fixed nitrogen is about 12 million tons per year, of which about 3 million tons are produced in the United States. The United States presently has a plant capacity of about 4 million tons annually. Following rapid growth in both productive capacity and consumption from 1947 to 1954, an imbalance developed in the industry which today gives a surplus capability over consumption of about 1 million tons of nitrogen per year. Industrywide, this means that 25 percent of installed capacity is not now being used. The long-range prospects, however, are an increasing market for present and many new uses. A total nitrogen demand of about 6 million tons in the United States in 1975 is estimated by experts in the field. Transportation charges, raw material availability, and market density govern the size of fixed nitrogen plants. Transportation by tank car adds about 6 to 7 percent to the cost for every 100 miles. Thus it is possible that there may be locations in the United States where chemonuclear plants may eventually compete with conventional plants. The chances are much better of course that there are places in other parts of the world where fossil fuels are limited and where transportation costs are high, where a chemonuclear plant that can produce fertilizer from air, water and uranium, may be economically attractive.

The use of a chemonuclear fixed nitrogen plant may also have implications in the mining industry. Today most ore treatment processes employ sulfuric acid, primarliy because of the considerable cost differential between sulfuric acid and nitric acid. The availability of

CHART 1

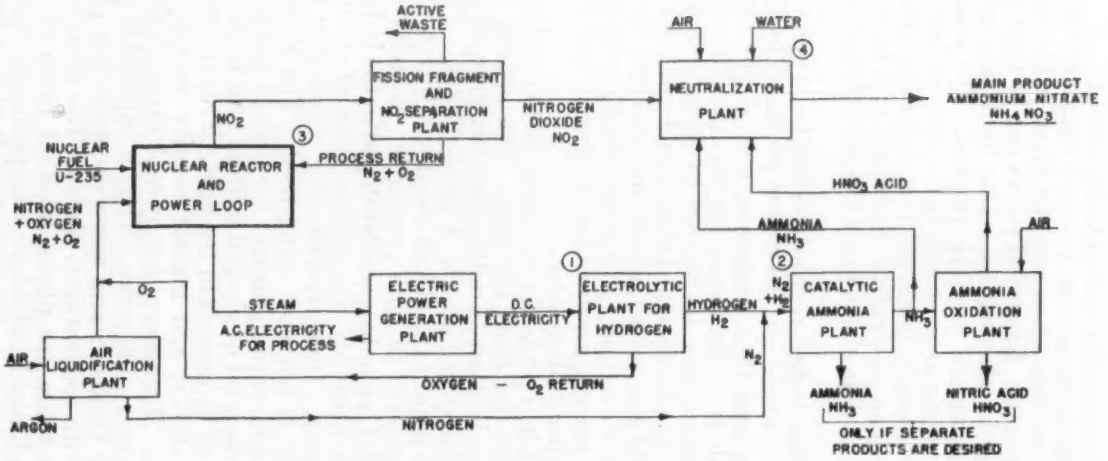
PROCESSES:

① $3/2\,H_2O \rightarrow 3/2\,H_2 + 3/4\,O_2$ — CONVENTIONAL PROCESS

② $3/2\,H_2 + 1/2\,N_2 \rightarrow NH_3$ — CONVENTIONAL PROCESS

③ $N_2 + O_2 \rightarrow NO_2 + 1/2\,H_2$ — NUCLEAR PROCESS

④ $NH_3 + NO_2 + 1/2\,H_2O + 1/4\,O_2 \rightarrow NH_4NO_3$ CONVENTIONAL PROCESS



FIXATION OF NITROGEN BY DUAL CYCLE CHEMO-NUCLEAR & CONVENTIONAL PROCESS

SINGLE PURPOSE–DUAL CYCLE FLOW SHEET
NUCLEAR–ELECTROLYTIC CYCLES

power, steam, and nitric acid directly at the mine source may permit the replacement of the sulfur-based process with the nitrogen-based process, particularly if the power or steam could be used to reduce the ore directly to the metal at the mine location. Presumably this would permit the more economic shipment of the metal rather than the ore, and would also possibly avoid sulfur stress or pollution problems. It would be interesting to study the applicability of such a scheme to the steel, uranium, aluminum, and other metal industries. We have not yet made such a study.

We have recently begun to evaluate the production of fixed nitrogen by yet another chemonuclear process. This one involves no nitrogen in the reactor system and avoids many of the safety problems in the system I discussed previously. It turns out that if carbon dioxide under certain conditions is exposed to nuclear radiation it decomposes to carbon monoxide and oxygen with a yield considerably greater than that found in the oxygen-nitrogen reactions. The carbon monoxide can then be reacted with steam to form hydrogen and one can once again convert the hydrogen to ammonia and then to nitric acid.

Other chemicals can also be made by chemonuclear processes. These are mainly organic chemicals. Although they may have considerable industrial importance in the United States at present, I do not feel they have as much national and international importance as fixed nitrogen. As examples, ethylene glycol, a material commonly used as an antifreeze agent, can be made from methanol. The Russians have paid considerable research attention to the radiation chemistry of phenol production from benzene, water, and oxygen. This case is interesting in that the yields appear to be high enough that a conventional nuclear fuel could be used in a reactor, and only the neutron and gamma ray energy absorbed rather than contaminating the chemical product with the fission products. Our present studies indicate, however, that both the ethylene glycol and phenol costs would be higher with a chemonuclear process than with conventional processes.

I would like to conclude with these remarks:

I believe that the development of chemonuclear reactors will be costly and time-consuming. I think we need several more years of study, experimentation, and evaluation to define all of the problems and ascertain the best technical direction for a development program to follow. I suggest also that the AEC and the people working in this field need guidance from the Congress and from the State Department as to how important the potential goals of the program might be. For example, how important is it for the United States to provide self-sufficiency in agriculture to an underdeveloped country by providing it with fertilizer plants?

I personally do not feel that there is any justification at the present time for a markedly accelerated program in this area. Rather, I feel that we should continue to define the problems and evaluate the potential with cautious reserve until a clear-cut advantage and application is seen for a chemonuclear process.

Representative PRICE. Thank you very much, Mr. Manowitz. I notice your reference to the fact that the Russians have paid considerable research attention to the radiation chemistry. I also note that you were a delegate to the 1959 Warsaw Conference on large scale radiation sources.

Mr. MANOWITZ. Yes, sir.

Representative PRICE. Did you get the impression there that the Russians were also active in other areas of radiation chemistry besides the one you mentioned in your paper?

Mr. MANOWITZ. Yes. It appeared to me that they were interested also in producing polyethylene from ethylene and were interested in rubber vulcanization. I think perhaps they have a different point of view than we have in approaching the development of a chemical industry. My personal feeling is that they have set themselves a requirement for getting into the chemical business in the relatively near future. Perhaps because they have a considerable number of technicians in the radiation chemistry field they feel they can buy time by using nuclear processes even if they are more expensive than conventional processes.

Representative PRICE. You asked the question in your paper how important is it for the United States to provide self-sufficiency in agriculture to an underdeveloped country by providing it with fertilizer plants. Perhaps the Russians are also asking the question, and they may be thinking of a nation like India.

Mr. MANOWITZ. That is possible.

Representative PRICE. What would your comment be on India where these types of plants might be very useful in the development?

Mr. MANOWITZ. I have read in the papers where there are plans for proposing fertilizer plants for India specifically, although the present plans, as I understand it, are for a conventional process.

Representative PRICE. You mean the Russians are?

Mr. MANOWITZ. No. I believe Mr. Garth of the United States has proposed this to the Department of State.

Representative PRICE. Do you think this might be a useful instrument in any assistance program we might want to enter into with a nation as vast and in many ways as underdeveloped as India?

Mr. MANOWITZ. It is a policy question not for me, but for other people.

Representative PRICE. I realize that. I am asking you whether you think this type of plant might be efficient and effective in that part of the world. It may not be here as you have now, but perhaps in that part of the world it could possibly be.

Mr. MANOWITZ. I would put it this way. If, indeed, it is decided to be a good policy to provide fertilizer plants in countries such as India, then I think one should consider a chemonuclear plant as well as consider other conventional ways of providing fertilizer plants, but that the economics should really determine which type of plant is installed, rather than the fact that we have an interest in nuclear energy.

Representative PRICE. It would more likely be economical in India than perhaps in this country.

Mr. MANOWITZ. I think that is correct.

Representative PRICE. Mr. Bates.

Representative BATES. Mr. Manowitz, you are one of these rare witnesses that does not advocate acceleration of his own program.

Mr. MANOWITZ. Yes, sir.

Representative BATES. Why are you so different?

Mr. MANOWITZ. At the moment I am just trying to advocate what I think is in the national interest, not necessarily in the interest of Brookhaven National Laboratory or myself.

Representative BATES. You indicate we should have a cautious reserve until the clear-cut advantage and application is seen.

Mr. MANOWITZ. Yes, sir.

Representative BATES. A good deal of that resolves itself around the cost factor; doesn't it?

Mr. MANOWITZ. Yes, sir.

Representative BATES. If you did have an accelerated program, you could determine at an earlier date its economic advantage; could you not?

Mr. MANOWITZ. At a somewhat earlier date; yes. However, I believe we can do an awful lot of good work with pencil and paper before we need go into an accelerated hardware program.

Representative BATES. You are satisfied with the program as it is presently progressing?

Mr. MANOWITZ. I believe I am, sir.

Representative BATES. Thank you.

Representative PRICE. Thank you very much, Mr. Manowitz. Would you also take a seat at another section?

The next witness will be Mr. Harry Perry of the Bureau of Mines.

STATEMENT OF HARRY PERRY,[1] CHIEF, BRANCH OF BITUMINOUS COAL RESEARCH, BUREAU OF MINES, U.S. DEPARTMENT OF THE INTERIOR

Mr. PERRY. Thank you, Mr. Chairman. The Bureau of Mines has been studying a number of subjects concerned with reactor development and with irradiation that might be of interest to this committee, and we are grateful for the opportunity to present this material to you. Our work in this field is divided into three parts: First, studies on the irradiation of coal; second, on high-temperature reactors for process heat; and, third, on the potential application of radioisotopes in the coal industry—although this last phase will not be discussed here today.

In the case of irradiation of coal, some experiments by the Bureau of Mines and others have been made to determine the effect of irradiation on the physical and chemical properties of various ranks of coal. The Bureau has a number of more extensive tests underway to measure more systematically the type and magnitude of these effects. The previous experiments have indicated that the effects were small and that changes in the coal structure would probably not significantly affect further processing of irradiated coal. According to information obtained to date, measurable changes in coal structure appear to consist of an increased number of molecular fragments and increased hardness, since massive irradiation of coal apparently makes grinding of the samples more difficult following irradiation.

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[1] Born September 1918 in Philadelphia, Pa. Received B.S. degree in chemical engineering from the University of Pennsylvania in 1940, and M.S. degree in 1941 from the University of Pittsburgh. Employed by the National Defense Research Committee during World War II and by the Department of the Army on explosives research. After service in the Navy from 1943 to 1946, began work with the Bureau of Mines. Presently, Chief, Branch of Bituminous Coal Research, Bureau of Mines, U.S. Department of the Interior, Washington, D.C. Member of Sigma Tau and Tau Beta Pi. Fields of study include mechanism of gaseous reactions, gasification of coal, and production of explosives.

At present, the Bureau has underway a series of tests to determine whether these preliminary conclusions can be confirmed and to develop new information on these and other physical properties. A bituminous coal and a lignite have been irradiated with gamma rays and preparations have been made for in-pile radiation of these and other samples. The effects of radiation dosage and time of exposure on a wide range of physical and chemical properties of coal will be determined. Within the limitation of available funds, as many different types and ranks of coals as possible will be tested.

There are no plans as yet to test for possible effects when coal is irradiated during processing. In order to accomplish this, relatively large expenditures would be required. The type of experimentation that will be necessary to develop this information has been outlined to the Atomic Energy Commission by the Bureau of Mines in a series of proposals submitted in February and November 1959. Phenomena that we believe should be investigated are: Effects of irradiation on nature of products and rate of reaction during (1) carbonization, (2) conventional hydrogenation, (3) high-temperature hydrogenation, (4) reaction of dry coal and steam at moderate temperatures and high pressures, (5) interaction of coal with oxygen or with steam alone, and (6) reaction of coal with other gases and with liquids.

We already have ample evidence that no major effects can be expected on the above processes if coal is preirradiated, by any of the methods tested to date, before being reacted. However, particularly in the case of carbonization and reaction with certain liquids, which require rather low temperatures and result in changes in molecular structure, irradiation during processing may have much larger effects.

Also of interest to this committee is research by the Bureau of Mines on a process-heat reactor. About 7 years ago, the Bureau suggested to the Atomic Energy Commission that the high temperatures, obtainable by nuclear means, might have applications in coal processing. The most obvious one is gasification of coal to make carbon monoxide and hydrogen not only for the production of organic chemicals but also to serve as the basic raw material for substitutes for natural gas and petroleum products. In conventional coal gasification, high temperatures and heat requirements are obtained by burning part of the coal with oxygen. The concept of using nuclear process heat might be applied not only to the gasification of coal but to other heat-using technology, such as carbonization and metallurgical processing. The AEC has cooperated with the Bureau of Mines in supporting research and development in this area. The temperatures required for these reactions are far beyond those being considered for the power program. In order to develop the necessary engineering information, the Bureau has constructed two systems in which heat from a nuclear reactor is simulated by electrical heating. One of these systems has been tested at atmospheric pressure and the second is being tested at 250 p.s.i. The objectives are to develop a reactor capable of heating helium to a range of 2,000° to 2,500° Farenheit, and to transfer such heat to process streams. These engineering loops have been used to learn how to design and construct a simulated reactor, how to transfer heat from spherical ceramic material to the gas stream, how to design pressure-tight systems at these high pressures and temperatures, how to develop compressors capable of

recycling helium at 1,000° F., and also for obtaining the heat-transfer information necessary to design heat exchangers for this severe service. Laboratory tests have been made on a number of materials of construction and on methods of removing fine dust at these elevated pressures and temperatures.

Based partly on this work, the Atomic Energy Commission intiated research and development, and plans to build a reactor to produce helium at temperatures up to 2,400° F. using unclad fuel elements.

Additional, more detailed information on these subjects has been prepared for your information and is submitted now for your record.

Representative PRICE. It will be included in the record of the hearing, at the end of your statement.

Mr. Bates.

Representative BATES. Just what are you trying to accomplish by irradiating coal?

Mr. PERRY. We would like to know if there is some way we can speed up the processes which we feel will be new outlets for coal. For example, we know we can make liquid fuels from coal with present technology. Unfortunately, the price is prohibitive. We hope that the irradiation during the processing of coal with hydrogen may lower the temperatures or pressures at which this would occur, and thus give us the opportunity to become competitive with natural petroleum products.

Representative PRICE. Thank you very much, Mr. Perry. I wonder if you would mind remaining for the panel discussion?

Mr. PERRY. I would be glad to, sir.

(The information referred to follows:)

I. IRRADIATION OF COAL

Exposure of coal to atomic radiation alters certain physical and chemical characteristics of some coals, whereas other coals appear to remain virtually unaffected. This is true for both in-pile and gamma-ray bombardment, at least up to the highest levels of irradiation used thus far. Most of these studies have been made with dry coal. Conceivably, irradiation of coal suspensions in reactive liquids or during processing where intermolecular bonds are being broken or altered—with or without the presence of reactive gases—could have drastic effects that are not apparent from treatment of dry coal. Following is a brief summary of pertinent work of which we are aware.

Scientists from the U.S. Geological Survey and the Bureau of Mines collaborated in an investigation of coalified logs subjected to the natural radiation from uranium over long geologic periods. In the case of coal, as the carbon content increases, the structure more nearly approaches that of graphite, but long-term exposure of the coalified logs to radiation by uranium apparently causes the carbon to combine in a different type of structure. This was reflected in the different densities, optical properties, and X-ray diffraction patterns exhibited by these coalified logs when compared with coal. The same results are not expected to occur in artificial irradiation, unless special conditions are employed, such as suspension in liquid solutions. The results show, however, that long-term, low-level irradiation profoundly alters the structure of coal, and in a manner different from that experienced by coal in normal "aging."

After irradiation in an AEC pile, examination of samples ranging in rank from lignite to low-volatile bituminous coal indicated very little change, if any, in the chemical properties. A boghead coal, thought to be composed of polymerized, long-chain fatty acids, showed a decrease in hydrogen and carbon contents after 10-day irradiation. This work, begun by chemists at the U.S. Geological Survey, was later extended by the Bureau of Mines to physical examination of the irradiated coals. It was found that the number of molecular

fragments (free radicals) contained in the coals increased markedly for the medium-volatile and low-volatile bituminous coals; a smaller change was observed in one lignite only. The infrared spectra of the irradiated samples were more diffuse than those of the unirradiated ones, indicating greater disorder in the structure; but the structural details were unchanged, so that no appreciable changes in rank could have occurred. Although no special hardness tests were performed on these coals, it was noted that grinding of the samples (to prepare them for infrared analysis) was much more difficult than grinding unirradiated samples. Thus, it would appear that the polymeric structure of the coal was altered by radiation, probably in the direction of increased molecular weights. Whereas the more highly polymerized portion of coal is probably less reactive, the newly created molecular fragments may make certain portions of the coal more reactive.

More than 30 samples of anthracite were irradiated for the Bureau of Mines by the Curtiss-Wright Corp., Research Division, Quehanna, Pa. Three dosages of neutron and three of gamma radiation were used. Irradiated and nonirradiated samples were compared as to the gases involved during radiation; other tests included porosity determinations, ease of oxidation, and electrical conductivity, to detect changes caused by radiation. Only the highest neutron flux caused any definite change, and this was an increase in hardness. Small changes in electrical resistivity were observed, but these may have been within the limits of error of the method of measurement. However, since only a limited number of tests were made to characterize the coals, other tests might have shown further differences. Thus, for example, an increase in the concentration of free radicals may have occurred but this has not been measured.

Further investigations of irradiation effects on coal are underway at the Bureau of Mines at present. Some results, obtained after irradiation with gamma rays at AEC installations, are now available. They show that irradiation has resulted in little, if any, change in the properties of a sample of a North Dakota lignite and one of Rock Springs bituminous coal. No changes occurred in their proximate and ultimate chemical analyses, heating values, carbonization assays, and infrared spectra. No large differences were found in the susceptibility to hydrogenation of the two coals before and after radiation; both appeared to be slightly less susceptible to hydrogenation, again indicating some possible cross-linking to form higher polymers, but it is not certain whether the results are significant. Determinations of other properties are still in process, including reactivity toward gasification, grindability, solubility, free radicals, surface areas and densities, and spectroscopic and X-ray examinations.

An exploratory 15-day irradiation of Rock Springs coal in the engineering test reactor at Idaho Falls has been completed, but the sample has not yet been opened for examination because it is still too "hot." It may not be "cool" enough for normal handling in the near future, in which case the capsule will have to be opened and the contents handled by remote control.

In addition to irradiation by various types of atomic particles and rays, the Bureau has also used other sources of irradiation to study the effects on coal. For example, two bituminous coals were irradiated for the Bureau at the Naval Ordnance Laboratory by a powerful X-ray source, the rays from which are similar to gamma rays. A small amount of gas, mostly hydrogen, was formed during the irradiation, and barely discernible differences in the susceptibility to hydrogenation were found. These differences may have resulted from cross linking of molecules during irradiation, giving increased molecular weights.

Other Bureau samples of coal were exposed to bombardment by electrons in an electrostatic generator at the Gulf Research Laboratories. No changes were observed in the infrared spectra or in the number of free radicals as a result of this irradiation, which simulated beta rays.

In a brief study, designed to investigate further the published claims concerning micronization of coal by irradiation, six coals varying in rank from lignite to anthracite were exposed to gamma rays. Under the conditions used by the Bureau, no effects were found.

Two accounts of non-Federal work, concerned with gamma irradiation of coal suspended in an organic solvent, are worthy of mention at this point: One study is being carried out at the Armour Research Foundation, the other was reported from Russia. In both cases, carbon tetrachloride was used as a solvent.

The objective of the Russian experiment was to extract germanium from the coal, and in this they were much more successful with the highest level of irradiation than with the lower levels of irradiation. The Russians noted, apparently as an afterthought, that there was "increased solubility of the coal, a fact which will undoubtedly be of great interest from the point of view of the chemical processing of coal." However, no mention is made of the extent of this effect. The results at Armour appear to indicate more than the usual amount of chlorination may have occurred, but further work is necessary to show the magnitude of such effects.

2. SIMULATED HIGH-TEMPERATURE REACTOR

The first step in the production of a vast array of organic chemicals, synthetic high-B.t.u. gas, or synthetic liquid fuels from coal is the manufacture of either carbon monoxide plus hydrogen or of hydrogen alone. Production, purification, and compression of gas together constitute the most expensive process step in the production of any of these materials, so that the overall economics of such processes are determined by the cost of this operation. In conventional systems oxygen is added to the mixture of coal and steam to produce this gas. It appeared that appreciable savings might be possible if the heat for the coal-steam reaction could be supplied by nuclear energy, at temperatures at which the reaction proceeds at reasonable rates. Heat available from nuclear sources at these high temperatures might also have other possible uses, for example, in reforming methane, low-temperature carbonization, ferrous and nonferrous metallurgy, oil refining, and production of chemicals such as acetylene. Preliminary cost estimates have shown that we can afford more for nuclear energy in the form of process heat than for power. Nuclear heat at a cost of 50 cents to $1 per 10^6 B.t.u. appears attractive for some processes.

The advantages of nuclear heat for these uses are: (1) No temperature limits except those imposed by materials of construction; (2) ability to produce heat at high pressures at no extra cost; and (3) in recycle operation, the advantage of no stack losses.

There appear to be three methods by which nuclear heat can be used as a process heat source. The first is the "direct method" in which the reactants are fed directly into the nuclear reactor and are exposed to the nuclear fuel elements. This presents many difficult problems, because the elements must be capable of withstanding the temperatures required and still contain the radioactive products. In addition, this method introduces a serious problem of radioactive pickup of the products as well as, in some cases, the problem of removing unreacted solids from the nuclear fuel elements. In the second method, one or more process streams are recycled and preheated in the nuclear reactor. These recycle streams carry the heat to a recycle reactor into which the fresh feed streams are introduced. Part of the product is then recycled. In the third or indirect method an inert gas, such as helium, is heated in a nuclear reactor and gives up its heat to the process streams by indirect heat exchange in a separate vessel. With our present knowledge, this is by far the simplest system for using the heat of a nuclear reactor at high temperatures and pressures.

The Bureau of Mines experimental program has been directed toward the solution of problems connected with the indirect cycle. The principal problems have been design of fuel elements, construction of a heat exchanger for 2,500° F. service, and design and construction of compressors for recycling helium at 1,000° F. In addition, information regarding heat transfer to helium under these conditions of temperature and pressure was not available and had to be obtained.

Two units, designated as induction-heated simulated nuclear reactor loops, have been constructed and tested, one for operation at atmospheric pressure, the second for service at higher pressure. While operational problems were being overcome in the atmospheric unit at 2,500° F. and heat transfer data for helium were being obtained for these conditions, a second loop was constructed, capable of operating at the same temperature but at 250 pounds per square inch. The operational problems at these pressures, of course, were increased, and methods had to be developed to prevent or at least minimize helium leakage.

Other problems, concerned with design and construction of the helium recycle compressor under these conditions, also had to be overcome. At the present time, over 1,800 hours of operation have been obtained, but it will be necessary to operate at even higher pressures. These studies by the Bureau of Mines have indicated a need for the development of even better mechanical devices, and work is continuing on this phase of the problem.

In connection with these developments, the Bureau has been testing materials of construction both for fuel elements and for heat exchanger service. A vast number of ceramic materials have been tested under a variety of conditions, and although none is suitable for this kind of service, some idea of the properties of these materials has been obtained and the temperature limits for their use have been found. The Bureau is continuing work on testing materials that may have application under these conditions.

In addition, the Bureau has been studying methods of dust removal, a serious problem at high temperatures and pressures. These studies have included determining the efficiency of and devising methods for cleaning metal filters, and designing electrostatic precipitators for high-temperature and pressure service.

Based on these Bureau of Mines data, the Atomic Energy Commission has nad a feasibility study made of operating an unclad-fuel-element reactor at high temperature. This study, made by the Sandia Corp., indicated the potential advantages of an unclad reactor as: (1) A possible increase in maximum temperature, (2) high burnup of fuel, and (3) the possibility of cheaper fuel fabrication and recovery. The Sandia Corp. pointed out certain unresolved problems with regard to operation; these are:

1. How to use a contaminated coolant in an indirect cycle system.
2. Some radioactive material with long half life will deposit on inside surfaces of the system, and its rate of deposition and means of removal must be determined.
3. The effect of radiation damage on graphite at high temperatures is unknown.

The feasibility study, however, showed that there were no reasons why such a reactor could not be built and tested, and Sandia Corp. proposed construction of a "Turret" reactor in which part of the fuel elements could be easily removed and replaced in the reactor daily.

3. POTENTIAL APPLICATIONS OF RADIOISOTOPES IN THE COAL INDUSTRY

The following information has been taken from a statement by Dr. Harold J. Rose, Bituminous Coal Research, Inc., made on February 25, 1960, before the Congressional Joint Committee on Atomic Energy. At the request of the Office of Isotopes Development of the Atomic Energy Commission, Bituminous Coal Research, Inc., had made a survey on "* * * the nature and estimated extent of possible applications of radioisotopes and related technology to the mining, transport, and storage of coal by the coal industry, and to storage, in-plant handling, and use of coal by major consumer groups * * *."

One of the suggestions made in this report is that radioisotopes could be used for better estimates of coal reserves. Many thousands of boreholes are being or will be drilled through coal-bearing formations for other purposes, and, if radioisotope logging could be provided at the same time, valuable information on the depth, thickness, and approximate quality of coalbeds that were penetrated could be obtained.

Isotopes might be used for keeping the cutting heads of mining equipment in the coal, close to the roof or floor rock, but without cutting into it. Such devices would greatly aid in preventing damage to alloy cutters and in extending the life of mining equipment. Similarly, radioisotopes could be used in preventive and repair maintenance, for the selection of optimum lubricants, etc., in connection with mining machinery. Radioisotope moisture and density gages might measure the load-bearing capacity of mine floors, and thus contribute to greater safety in mines. Similarly, radioisotope tracers are being considered for studying mine ventilation problems, to avoid explosive mixtures of air and methane gas. Isotopes should also be considered for the study of coal dust problems and to facilitate dust collection in mines. Radioisotope-energized lights, which would function independently of the mine light circuit,

could find use as warning agents on moving and stationary equipment. To aid in the prevention or abatement of water pollution, radioisotopes could be used to trace the source of water entering mines, leading to the control of inflow and alleviating acid-mine-water discharge.

In coal preparation, radioisotopes are already being used by a few coal producers and users in the United States for various purposes, such as automatic control of the density of heavy-media suspensions used in cleaning coal; automatic control of the most efficient solids content of coal slurries in cleaning plants; determination of density of coal at various depths in storage piles, for stockpile inventory; indication of stoppage of flow of damp coal-fines from overhead bins to boilers; and as tracers in hydraulic transportation of coal in pipelines. Further suggested uses are controlling the operation of coal-cleaning jigs, thickeners, etc.; determining the particle size and settling rates of —200-mesh coal slurries; determining the selectivity, coverage, effectiveness, and ultimate fate of flotation reagents, flocculants, dustproofing, and freezeproofing compounds; continuous weighing or analyzing of moving coal streams; and studying procedures for coal crushing and screening.

In transportation and handling of coal, radioisotopes might find application for many of the same purposes, such as continuous weighing and pipeline transportation. In addition, they might be used for checking the condition of belt carcasses, to monitor automatic loading of cars, and to measure the amount of coal in bins.

Rapid continuous methods for testing coals are needed, and radioisotopes might be applied to the solution of these problems; for example, in the analysis of a whole coal stream or an aliquot portion of it, isotopes might aid in obtaining immediate test results. These methods would also save time and cut costs in laboratory procedures.

Additional uses for radioisotope techniques could be developed in connection with the utilization of coal; that is, primarily its combustion and metallurgical uses.

To quote Dr. Rose, "Most of the numerous possibilities for radioisotopes which I have mentioned are still only ideas or suggestions. Much basic research and engineering research will be required to definitely establish the technical possibilities. This is due to the fact that coal is a natural product which varies greatly in rank, type, and purity in various deposits."

Many of these uses of isotopes are of immediate interest in connection with the Bureau's coal programs. We have already applied isotopes as tracers in pipelines and are considering their suitability in connection with acid-mine-water and gas-drainage studies. In addition they have been useful in research on the mechanism of coal gasification and of synthesizing fluid fuels from coal-derived gas. These examples, though indicating actual and potential applications of isotopes, represent but a small sampling of the varied areas in coal research and development in which the Bureau of Mines has found isotopes to be of use and expects to apply them in the future to some of the problems discussed above as well as others connected with mining, sampling, analyzing, preparing, upgrading, and using coal.

ROUND TABLE DISCUSSION

Representative PRICE. The following persons will gather around the table for the panel discussion on the advanced reactor application: Dr. Cohen, Dr. Hall, Mr. Manowitz, Dr. Spinrad, Dr. Chauncey Starr of Atomics International, Dr. Alvin Weinberg, Oak Ridge National Laboratory, Dr. Walter H. Zinn, of Combustion Engineering Co., and Mr. John W. Simpson, Westinghouse Electric Corp.

We have many distinguished scientists at this session this morning. I would like to start the discussion by proposing one question, and I think you can take it from there.

I would like to pose this question: What should we be doing in the field of advanced reactors which we are not doing? I would probably like for Dr. Zinn to start off the discussion.

Dr. ZINN. I must first say that I don't feel completely qualified to answer, because I don't know what we are doing in detail. I believe one common factor of the advanced reactors that were mentioned this morning probably is that they are high temperature devices. I include the process heat reactors we heard about last, and the compact reactors we heard about from Dr. Hall and Dr. Spinrad. It seems to me that if we are really going to talk about high temperature reactors, we are taking a very bold step and we need to implement the pencilwork and paperwork, which was mentioned, with laboratory work.

I would suggest that before we can take seriously really high temperature reactors, we must work with the basic reactor materials in the high temperature environment. We need to irradiate fuels at high temperature, which we have not done to any extent. We need to cope with a problem which I think is characteristic of all such high temperature devices, namely, that the fission products which we are ordinarily content to leave in the fuel elements won't stay there when the temperatures are high and will enter the coolant streams or some other atmosphere. This will give us a new problem in maintenance, tightness of systems, and perhaps safety problems.

I would suggest, therefore, that the simplest answer I can give to what aren't we now doing is that we should do more experimental work with our research and test reactors to find out if, indeed, we have much chance of such high-temperature operation. I don't think very much along these lines is now being done.

Representative PRICE. Dr. Hall, I wonder if you would want to comment on the fission products in the stream?

Dr. HALL. There is a reactor experiment underway at Los Alamos Scientific Laboratory, which was mentioned briefly in connection with the coal gasification problem, which faces up to this problem that Dr. Zinn just mentioned. We are planning to run a graphite moderated reactor with unclad fuel elements at gas temperatures up to 2,500° Fahrenheit. In doing this, we are allowing the fission products to escape into the gas stream. This approach is not guaranteed to be successful by any means and is truly an experiment. I think this experiment will do much in telling us just how bad a headache a contaminated coolant gas will be. It will be a very serious problem, but we have to find out at some time whether we can live with it.

I am also aware that at ORNL there are experimental loops which are approaching the same problem. These two experiments are very close together in time and are the subject of mutual collaboration between the two Laboratories.

Representative PRICE. Dr. Starr, I wonder if we could have some comment from you as to what you think we should be doing in the reactor field.

Dr. STARR. I would like to support Dr. Zinn's preliminary remark. The big advancement in nuclear technology should come from the movement into high-temperature reactors. We really have three types that are under development in the country now. The turret

reactor to which Dr. Hall referred is in a neutral atmosphere. The Rover project is in a reducing or hydrogenous atmosphere, and the Pluto project is in an oxidizing atmosphere. The chemical problems of the high temperature fuels in these atmospheres are somewhat different.

If we are going to move into this high temperature field, then work on very high temperature materials and fuels becomes the crucial effort. I would also agree with Dr. Zinn that much more experimental work in this area ought to be done.

In addition, I would like to discuss the possibility of direct conversion. Direct conversion is ordinarily understood now to be the conversion of heat into electricity without the use of rotating equipment, turbines, and generators. This is a process which has been experimented on in various ways. Dr. Spinrad mentioned that he didn't know of a reactor project utilizing this approach. This is perhaps not a complete statement of the situation in the atomic energy scope of work. As part of the Snap program for space nuclear powerplants, there is a study that has been going on for some time now on the application of a direct thermionic converter to a nuclear fuel element. The specific reason for this application in space is that it permits a very much more compact plant than one utilizing rotating equipment, and has other advantages which go with the absence of bearings and other devices which may not have a high degree of reliability.

I would like to comment on Mr. Manowitz' comments on chemical applications. I believe that one of the big lacks in this field is one which he mentioned; namely, the absence of some fundamental knowledge in the way chemical reactions are affected by radiation. We, as almost everyone else in the reactor business, have discussed with Mr. Manowitz and among ourselves the possible applications of nuclear radiations. One of the big gaps is to find the chemical processes and how they are affected.

I think that here again we need some fundamental research in the basic chemistry of what radiation does to chemical reactions. I don't think the engineering problems are insuperable.

Representative PRICE. Do you think we are doing enough research right now on the basic chemical radiation?

Dr. STARR. As far as I know about the work in radiation chemistry, I don't think it is enough. I think much more ought to be done. But this is typically the kind of thing that is done in universities. It is a very fundamental type of scientific kind of work. We just don't know enough about the kinetics of chemical reactions as affected by radiation. I think until we do we are not going to find that area where nuclear radiations are going to be particularly valuable. The fact that such an area might exist is a hope which I think we all have.

Representative PRICE. Thank you very much.

Mr. Ramey?

Mr. RAMEY. I have a question for Mr. Simpson. Last evening at the American Nuclear Society one of the speakers I think listed eight advantages or characteristics of reactors of the advanced types. An

AEC representative there commented informally that after they had gone through the eight listed that he left out one, the ninth one being that the Atomic Energy Commission should pay for each of these advanced concepts.

Do you think that industry has a role in developing these advanced concepts with some private participation?

Mr. SIMPSON. I think that undoubtedly industry does have a role. However, the more advanced concepts will necessarily have to be funded in most part by the Government. I would like to add in addition to that statement something to what Dr. Zinn and Dr. Starr have said about the high temperature phase of reactor work.

In the past, we have had considerable fallout of technology from one program to another. For example, the work on the naval reactors program has helped the power program. The Atomic Energy Commission is doing work, as has been mentioned, on a nuclear rocket engine. This engine is a very high temperature device. While it bears little resemblance to today's power reactors, as I look into the future at reactors to be used in a magnetohydrodynamics system for example, where they depend upon very high temperatures I see a relation. When coupled with a high temperature nuclear reactor as an energy source, a magnetohydrodynamic system would appear to be very attractive for getting high performance and efficiency. Of course, this is quite a way into the future. I can only congratulate the committee and the Commission on doing this work, and I would urge that anything that they could do to push this type of work in the high temperature systems that could have a fallout into future power programs would be beneficial.

Representative HOSMER. Mr. Chairman, I would like to observe that the last two questions have brought a third that is not the subject of discussion today, but we should get into it at some time, probably, and it is the relative amount of effort that we should direct toward just plain basic research in some of these more exotic applications.

I would like to ask Dr. Weinberg a question that has been on my mind for some time. You have been doing work with the liquid fuel. Has any thought been given to a centrifugal separation of fuel regions within the liquid fuel reactor?

Dr. WEINBERG. You mean within the reactor to keep the two regions separated?

Representative HOSMER. Yes.

Dr. WEINBERG. A little thought has been given to it but it seems rather difficult because one would have a dynamic equilibrium which if disturbed would affect the reactivity and the power would be subject to violent fluctuations. We do use centrifugal method for separating outside the reactor.

Perhaps, Mr. Chairman, I might take this occasion to make a few remarks, if I may.

I would like to say that I was rather disappointed that in the list of advanced concepts no mention was made of the system which I consider to be the most important of the advanced concepts, and upon which I believe the whole future of nuclear energy depends, and these are the breeder systems. I cannot stress too strongly how very deeply

I feel that if nuclear energy is really to amount to something in the real long term, if it is to justify, if you like, the large sums of money which we as a Nation and the world at large spend on nuclear energy, then one must succeed in exploiting fully the uranium and thorium that is present not only in the high grade materials but in the low grade materials. This bears on the remarks of some of the previous speakers who point out that central electric power generation represents a small fraction of the total energy economy of the world. But I believe this is temporary. We have estimates, as you know, which are on the whole fairly reliable, and which indicate that as one runs out of the fossil fuels, and as one runs out of real ores in other materials, the energy economy will depend more and more heavily upon cheap central electricity. We have estimates that indicate that within the next 100 years one can easily get 70 percent of the total energy of the world one way or another going through central electricity.

I would, therefore, say in answer to the question, Do you think we should be doing more in the way of advanced concepts? The one concept I would like to see more work going into is the breeder.

I have only one other point to make. I would like also to call to the attention of the committee the fact that if we ever do make of nuclear energy a really important energy source, if it does represent a large fraction of the total energy economy, then the problem of waste disposal is going to become an extremely difficult and important one. I would urge that full attention be focused upon this as the second of the two central problems in nuclear energy.

Representative PRICE. Dr. Weinberg, I have been under the impression that we do have a substantial breeder program going on. I would like a further comment on that if you think there are some weaknesses in this program, particularly in the breeder, because we have with us the gentleman who built the first breeder reactor and who is somewhat of an authority on it. Would you care to make further comments, Dr. Zinn, on this point?

Dr. ZINN. I don't disagree with Dr. Weinberg. I just disagree on the meaning of what is meant by "the long run." When we first started to work on breeders in 1946, approximately, our estimates of the available supply of raw material, that is uranium, at a reasonable price were very, very much lower than what they are now. I think the situation now is that the reactors that we are building as central-station powerplants will obtain about 5,000 megawatt-days per ton of mined uranium, a fairly miserable figure. If we move into a large-scale nuclear power fission economy on that basis, we would use up our low-cost reserves in a rather few years—let us say 25 years.

However, we do see possibilities of improving these reactors and adding to them reactors which can be made with existing technology, which would raise this usage of the mined uranium to about 20,000 or 25,000 megawatt-days per ton. As soon as we have done that we have extended the usage by a factor of 4 or 5, and in addition we make available 10 times more raw material which is slightly more expensive uranium. As a result of this, we then have enough material to fuel a fission nuclear power economy for perhaps 200 years,

at which point we most certainly would have to have breeders which can get a yield of approximately 200,000 megawatt-days per ton of mined uranium. I don't know what these breeders will be like. I don't know whether they will be made with any of the fuel systems that we are now talking about. I believe one point has to be remembered very much. These systems will require a lot of uranium for inventory, and if this inventory comes from expensive sources, such as the second source I mentioned, the inventory cost may be a bit too much. So we cannot really wait hundreds of years. We have to get the breeder system going while we can inventory it with cheap uranium or the low-cost uranium we have now. That means we should have breeders in the neighborhood of a hundred years or somewhat sooner. I guess that is where I begin to agree with Dr. Weinberg. I feel that the most important thing now is to get an industry going based on economically competitive reactors, whether these are breeders or not. The various advances which surely will ensue will show us how to make the breeder reactors.

Representative HOSMER. I was going to ask if Dr. Cohen would comment.

Representative PRICE. I was just about to call on Dr. Cohen for comment.

Dr. COHEN. I am very happy to be able to take up Dr. Zinn's last remark here and give my full approbation to it. I might say I have been brought up to believe, and do believe, that economics is the epitome of the conservation of all the elements of a system—not just one of the many things. Resources, men, energy, iron and steel, equipment, cement, and so forth. I believe that if you use good economics you will take care of the worries which the other two gentlemen have been bringing forth in due course, and as it is appropriate to do so.

I would like to make another comment on the question of what should be done to accelerate the programs that are needed for the long range. I might observe that the long range in atomic energy is away beyond 10 years. Ten years in this field is a short range. It is not always true that a large program and a large hardware will result in the quickest rate of progress. In fact, often the opposite is true. Large steps can be done on a laboratory scale, or with a pencil and paper by somebody dreaming and thinking about it. While I have been told by people who know more about the Sherwood program than I, and this is therefore hearsay, that more would have been gained if there had been less early large hardware, I suggest that the area of developing technology as a whole—all of the nuclear technology—is the surest base that we have for substantial and rapid progress.

Representative PRICE. Thank you very much, Dr. Cohen. Does anyone else care to comment?

Dr. WEINBERG. I realize that this question of breeders versus burners goes on interminably. I would like to make clear in my opinion the ultimate objective of large scale fission reactor development is the economical breeder. I believe it is something that can be done

with a small extrapolation of present day technology. I do not believe that we in this country nor in the world as a whole quite spend enough time and effort at hitting the $64 question jackpot, which is the development of a breeder which is economic. I think it can be done.

I would, Mr. Chairman, like in this connection to submit for the record a paper which I wrote on this general subject.

Representative PRICE. Without objection it will be included in the record, at the end of our discussion.

(The information referred to appears on p. 194.)

Representative HOSMER. Dr. Weinberg, in relation to the amount of uranium that we will be burning up in the next 25 years, how urgent is it that we get this breeding business in hand?

Dr. WEINBERG. If the breeding business can be shown to be as economical as the the nonbreeding business, then there is no argument against making the breeders soon.

Representative HOSMER. I don't mean from that standpoint. I mean it from the standpoint of wasting an irreplaceable resource.

Dr. WEINBERG. Nuclear energy in the next 25 years is not going to be a very important energy source anyhow, and therefore bears little relation to either the reserve of uranium or of coal during that period. Therefore, whether we work on the thing which is not going to be the long-term solution or work on the thing which is the long-term solution doesn't make too much difference over the next 25 years. However, it makes a vast difference over the very long run, and I think, from the standpoint of the overall national interest, it is the very long run which counts most.

Representative PRICE. I would like to pose this question for the panel. During Dr. Spinrad's statement he made a suggestion for a possible development in the future of large power reactors for use on the Moon and Mars and possibly other planets. Considering the leadtime required for such development, what should we be doing on such an application? Who would like to start off with that? I would like to make a prediction here. Some of us have been interested in the military expenditures, what they need to fulfill requirements of power on Okinawa and Guam; we will have one on the Moon or Mars before this becomes a reality. Mr. Simpson?

Mr. SIMPSON. There is a program for powerplants that will operate in space; this is the SNAP program. The logical thing to do to get toward the larger powerplants that could be useful on the Moon is simply to start with larger Snap programs and have one at 1,000 kilowatts as the next step and work on up.

Representative HOSMER. Let us take that question from the reverse side. The Army wants to get some kind of atomic energy propulsion for land vehicles, and whether that is a reasonable thing to work on.

Mr. SIMPSON. The type of reactor you would use for a land vehicle would not be particularly suitable for a base on the Moon because of the difference in the environment.

Representative HOSMER. I realize that. From the standpoint of whether we should be fooling around with it at all or doing something that is a little more immediate in application is the question.

Mr. SIMPSON. That is a question of military strategy that I am not competent to answer.

Representative PRICE. Dr. Spinrad.

Dr. SPINRAD. I would like to essentially make a synthesis of that question and some comments I have been storing up during Dr. Cohen's last remarks. I think that we have here a case where a large program, as such, as a development effort is not indeed called for now, but can be anticipated in the future. What is needed is a sufficiently good broad program not only in basic research—we know most of the phenomena of this sort—but what I will call basic engineering research, which does not require large expenditures of money in a project form but does require very tight allocation of funds in the sense of making sure that people who need some useful piece of data can go get it. I can take such examples as the fact that in studying very high temperatures coolants we discovered a complete lack of thermodynamic data about the vapor that could be used. It is this sort of problem area to be explored at this time. Materials must be explored, too, of course.

Representative PRICE. Dr. Starr.

Dr. STARR. The question was asked, what relative effort should go into this type of very long range application, specifically the development of powerplants to use on the surface of the Moon. The question of whether one sets up a colony of men on the Moon is obviously one of broad national significance and it is certainly not within our province here to determine whether it is an objective for the Nation or not. But if it is, I would give as a rule of thumb that certainly 10 to 20 percent of what one is spending on getting a colony to the Moon is worth spending on the power source for that colony. To keep a group of people or even one man on the moon's surface for any length of time involves such logistics of supply, principally for energy requirements, that it becomes a rather serious problem.

I would say that if in any of these outer space projects we can justify a certain number of dollars for the development of the project, certainly 10 to 20 percent of that ought to be devoted to the development of an energy or power source for that application.

Representative PRICE. Dr. Zinn.

Dr. ZINN. I would like to pursue that thought in a slightly different way. We have given the impression here that if we are going to make these compact transportable reactor systems we must do high temperature research. That was touched on, and I agree. I don't want to give the impression for a moment that I believe we can come close to accomplishing it with a rather small extension of research effort. I think the committee is fully aware of how much money has been spent on getting a nuclear propelled airplane.

Representative PRICE. We are very well aware of it.

Dr. ZINN. I would hazard the guess that to get a nuclear powerplant which could supply the kind of power that Dr. Spinrad mentioned, on a planet, would require an expenditure of money which would make the aircraft program look a little small.

Representative PRICE. But we will have it before we have the aircraft.

Dr. ZINN. That is possible. We are starting in a little different way.

Representative PRICE. Senator Anderson.

Chairman ANDERSON. I am wondering what the prospects of the use of plutonium as a fuel might be. Doesn't a large part of our future depend on it? I thought Dr. Hall might comment on that. I know he has been working in that direction.

Dr. HALL. The question of utilization of plutonium is directly connected to the statements which Dr. Weinberg has made before, i.e., the necessity of using the full content of the uranium energy in the reactors if anything is to be made of nuclear energy in the long-range picture. We at Los Alamos are working with plutonium enriched fuels as one approach. This is not the only approach which is being explored. The breeders which are being developed, and, indeed, almost any reactor will produce surplus plutonium which must be utilized. I think this is a very expensive line of research but one which must be pushed forward.

Representative PRICE. Does anyone care to make any specific comment on any of the testimony here this morning outside of Dr. Weinberg feeling that we should put more into the breeder program? Are there any other comments relative to advanced concepts that were left out of the presentation so far this morning? Dr. Weinberg.

Dr. WEINBERG. I have only one general comment which I would like to make. In listing the long term possible applications of nuclear energy, one necessarily finds in the list a whole spectrum of possibilities, ranging from those things which we believe are technically feasible to those things which are very far from being technically feasible. I would hope very much that the Joint Committee and the Commission in considering the possibilities of long-term reactor applications would keep very carefully in mind the fact that because a system is in a list of possibilities, this does not by any means mean that the system is feasible or will ever come to fruition.

Representative PRICE. Dr. Spinrad.

Dr. SPINRAD. I would just like to add one comment on the pace of development which has been touched on by some of the questions. It seems to me that there is a bit of confusion about the actual use of the term of development. In going into the development of reactor systems, in brief, there is a very close connection between the phase which starts off in the laboratory at the bench and goes on through to an experimental reactor. Beyond that, the development is so different as to constitute almost a completely different phase. It is the translation of technology which has now been demonstrated into technology which is feasible on an industrial scale.

I think if we separate these two phases of development we would have a clearer picture of where an advanced concept ends up and a system under development for current usage starts.

Representative PRICE. Dr. Hall.

Dr. HALL. I would like to add to the remarks that Dr. Weinberg just made since I included a number of what might be called speculations as to the utilization of reactor power in my presentation to you earlier. I agree wholeheartedly that these things should not be taken

as objectives to be made into programs at this time. I think the only point on which I would differ with Dr. Weinberg is the impression of the ultimate importance of the central power station. It is my feeling that as we develop reactor fuels which are more efficiently used and cheaper in all aspects, that the base of applications will become broader and will be applied in fields which have not even been touched on in the present discussion. I think we share the same common objective that we must reduce the fuel cycle cost of reactors and the applications will then follow automatically.

Representative PRICE. Are there any questions by any members of the committee? Mr. Hosmer.

Representative HOSMER. I better refrain from asking this question because it might start a little controversy. The question would be to each of you, if you had your own choice of the decision to make, which of these advanced reactor concepts would you spend the money on and go ahead with?

Representative PRICE. Mr. Bates.

Representative BATES. I would like to ask Dr. Weinberg whether he thinks that our present emphasis is proper. In other words, are we putting too much into construction and not enough into long range advanced planning, or whether you think the present program is at the proper level?

Dr. WEINBERG. I am afraid I tend to sound like a broken record, Mr. Bates, in that I repeat myself over and over again. I do believe that the world's program, not only the U.S. program, in atomic energy is somewhat too heavily geared toward the burner type reactors and should be pushed a little more toward the breeder type reactor. Not only breeders, but economic breeders. I should say also that the waste disposal program should be given more attention than I believe it receives in this country or in other countries.

Representative BATES. Do you think there are any particular areas you feel we should eliminate? You have been asked whether we have been doing enough in some fields. I will ask the converse of that. Do you think we are doing too much in certain fields?

Dr. WEINBERG. Mr. Bates, that is a very complicated and loaded question.

Representative BATES. If you want to pass it, it is all right.

Dr. WEINBERG. All I can say is that relative to what else is going on the breeder should be given a bigger play.

Dr. STARR. I would like to comment on Mr. Bates' question. I don't quite agree, as I think is well known in the trade, with Dr. Weinberg's position completely on breeders, but I feel that there is a problem of distribution of type of effort which goes on in the atomic energy activities. I believe from the long-range point of view we learn more and make greater progress if we put more emphasis on what is called fundamental research, the building of reactor experiments, the learning of information and the testing of ideas than we do from building very large operating plants.

On the other hand, from the short-range point of view, unless you get into the big hardware, you don't get operating plants. So I think here is a policy situation which is in a sense almost nontechnical and

is really one of national policy. From a long-range point of view I would put the emphasis on research and reactor experiments. From the short-range point of view, if you want to get operating experience quickly, you have to put the emphasis on hardware which is about where it is now. The biggest emphasis now is on big plants.

Representative PRICE. Thank you very much. On behalf of the committee I want to express my appreciation and thanks to the various people who have testified here this morning and participated in this panel. It has been an honor and privilege to have you before us.

The committee will stand in recess until 2 o'clock this afternoon, when we will meet on the subject of "Space Propulsion and Power."

(The material referred to on p. 189 and other supplementary material follow:)

THE PROBLEM OF BURNING THE ROCKS

Alvin M. Weinberg

In his provocative, but dismal, book "The Next Million Years" Sir Charles Darwin predicts that man is probably doomed to spend the rest of his days in an endless and bitter struggle for existence. As the population grows, and as raw materials, especially energy, become scarce, there will be less and less of everything to be spread among more and more people. Sir Charles sees only one possible way out of this dreadful and ultimate Malthusian impasse: if controlled fusion of deuterium were feasible, then the essentially infinite resource of cheap energy in the heavy hydrogen of the sea should make economically practical the conversion of energy into most of the other commodities needed by man. In that event the Malthusian impasse could be forestalled for a long, long time and perhaps avoided altogether.

Prompted by such intriguing long-range prospects, the scientific world has, since about 1950, been engaged in a large-scale effort to learn how to "burn the sea" - that is, to cause controlled nuclear reactions involving deuterium to proceed on a large scale. Although much has been learned about the high-temperature, gaseous plasmas, which are needed to sustain nuclear reactions based on deuterium, it would be quite wrong to assume on the basis of what has been learned so far that large-scale controlled fusion is feasible.

Fortunately this does not doom mankind to the desolate, energy-poor existence contemplated by Sir Charles. For, even ignoring the possibility of solar energy, let alone controlled fusion, we now have almost at hand, by means of nuclear breeder reactors, a way to tap an energy resource about as large as is contained in the sea. This energy resource exists in the huge residual amounts of uranium and thorium dispersed throughout the rocky crust of the earth. If breeder reactors are successful we shall be able, literally, to "burn the rocks", and thus provide mankind with an infinite and economical energy source, even though we never learn how to burn the

sea. The realization that the almost infinite energy source contained in the rocks is very probably available has come astonishingly late, largely as a result of work by Harrison Brown and by Farrington Daniels and their collaborators on extraction of trace radioelements from granite, and as the result of progress in solving some of the technological problems of breeder reactors. The recent establishment by the Atomic Energy Commission of a long-term program aimed at breeding, using thorium as a raw material, therefore comes just at the time when the full significance of breeding has been recognized. It will be my purpose in this paper to describe where we stand in the task of making breeder reactors practical, and to explain why nuclear breeding will enable us to use the vast reserve of dilute uranium and thorium in the earth's crust as an economic energy source.

General Description of Breeder Reactors

The basic energy production step in breeder reactors is the fission of U^{233} or Pu^{239}. Each fission is accompanied by the emission of about two fast neutrons; the average number of neutrons produced per fissile atom destroyed is denoted by η. One of these neutrons is needed to maintain the chain reaction, and the remainder (that is, $\eta - 1$) are available for manufacturing new fissile atoms by interacting with Th^{232} or U^{238} according to the reactions

$$n + Th^{232} \rightarrow \underset{\substack{\beta \text{ decay} \\ 23 \text{ min}}}{Th^{233}} \rightarrow \underset{\substack{\beta \text{ decay} \\ 27 \text{ days}}}{Pa^{233}} \rightarrow U^{233} \qquad (1)$$

in the thorium cycle, and

$$n + U^{238} \rightarrow \underset{\substack{\beta \text{ decay} \\ 23 \text{ min}}}{U^{239}} \rightarrow \underset{\substack{\beta \text{ decay} \\ 2.8 \text{ days}}}{Np^{239}} \rightarrow Pu^{239}$$

in the uranium cycle. This latter reaction is the one used to manufacture plutonium in the Hanford reactors. The isotopes Th^{232} and U^{238}, which are the common forms of thorium and uranium, are called "fertile". A fertile isotope is converted into a fissile

one by absorption of a neutron. If η, that is the number of neutrons emitted, is two or greater, then at least one neutron is theoretically available to manufacture, by interacting with a fertile isotope, a fissile atom. This new fissile atom can be used to replace the atom destroyed in the original fission, and thus there is no net loss of fissile material. A measure of merit of a breeder reactor is the "breeding ratio", i.e., the ratio (fissile atoms produced/fissile atoms destroyed). If the breeding ratio exceeds unity, the breeder reactor will produce more fissile material than it consumes (hence the name "breeder"); if the ratio is less than unity, fresh fissile material from the outside must be fed into the reactor to keep the system going. Since in a breeder reactor with breeding ratio of one or better, there is no net loss of fissile atoms, the raw material which is finally used is the fertile, and very abundant Th or U^{238}, rather than the much rarer and far more expensive fissile materials. Although in theory a breeding ratio of unity could be achieved if η is exactly two, there are always losses of neutrons to control rods, fission products, and structural material, as well as losses of newly-bred fissile material in chemical reprocessing, and it is generally believed that η must be at least 2.2 in order for breeding - i.e., a breeding ratio of ≥ 1, to be practically attainable.

A more significant figure of merit of a breeder reactor is the doubling time - that is, the time required for the inventory of fissile material in the reactor to double itself. The reciprocal of the doubling time (the "interest rate") is the product of two factors. The first is simply the breeding ratio, which is a measure of the number of new fissile atoms produced per fissile atom destroyed. The second is the specific power - that is the amount of power in kilowatts extracted per kilogram of fissile material contained in the reactor and its appurtenances. The specific power measures the rate at which fissile material is burned in the reactor. Since the burning of each atom of fissile material results in a certain fractional gain in over-all fissile inventory, the faster the fissile material is burned - that is, the higher the specific

power - the faster the inventory will be doubled, and the shorter the doubling time.

Thus we see that breeder reactors ought to have both high η and high specific power. The first quantity, η, is determined by considerations of nuclear physics, the second, by considerations of technology. We shall examine these two factors in turn.

We have seen that breeding is possible, in principle, either in the thorium-U^{233} cycle, or in the uranium-Pu^{239} cycle, provided the crucial quantities - η for U^{233} and η for Pu^{239} - are sufficiently high. Since the values of η are, in a sense, central to the whole long-term future of nuclear energy, a great deal of work at all the atomic energy laboratories of the world has gone into their measurement, especially the measurement of how η varies with the energy of the neutrons inducing fission. It is now well-known that the average number of neutrons produced in a fission event - a quantity called ν - is comfortably above two: it is, for fission induced by slow neutrons, $2.52 \pm .02$ for U^{233} and $2.91 \pm .04$ for Pu^{239}. Yet there has been serious question about achieving a breeding ratio well above one, at least in the Th-U^{233} cycle, since fissile atoms on absorbing a neutron do not always undergo fission; there is a competing process, discovered during the war by B. McDaniel at Los Alamos, consisting of simple capture with the emission of a gamma ray. This process is

$$U^{233} + n \rightarrow U^{234} \qquad\qquad Pu^{239} + n \rightarrow Pu^{240} \qquad (2)$$

and the resulting long-lived even-neutron, even-proton nuclides (U^{234} and Pu^{240}) are not fissile. Hence whenever a radiative capture process such as (2) occurs, a fissile atom is lost without producing additional neutrons. Now the quantity η is the number of neutrons produced per fissile atom destroyed - either by fission or by capture; hence if the relative probability for capture in process (2) is high, η will be small and vice versa.

THE PROBLEM OF BURNING THE ROCKS

In general, the competition between fission and capture tends to go more and more in favor of fission as the energy of the neutron which induces the reaction increases, although the ratio of the probabilities of the two processes does undergo sharp fluctuations at specific "resonance" energies. At thermal neutron energy - that is, the energy of thermal agitation which is about .025 electron volts or 290 ^{o}K - the ratio of capture to fission in U^{233} is 0.105 $\pm$ .007 and in Pu^{239} it is 0.39 $\pm$ .03. Hence the number of neutrons produced by absorption of a thermal neutron is, as a little algebra shows, $2.52/(1 + .105) = 2.28 \pm .02$ in U^{233} and is $2.91/(1 + .39) = 2.09 \pm .02$ in Pu^{239}. The value 2.28 in U^{233} is generally believed to be a sufficient margin to make thermal neutron breeding possible in the U^{233}-Th^{232} cycle, whereas the value of 2.09 in Pu makes thermal breeding in the U^{238}-Pu^{239} cycle very marginal indeed. At the neutron energy which would exist in a reactor which contained no light element as moderator - say around 500 kilo-electron-volts - the values of η are roughly 2.4 for U^{233} and 2.7 for Pu^{239}. The general course of η as a function of energy is shown in Figure 1. Thus in the Th-U^{233} cycle there is little nuclear advantage to using high energy neutrons to carry the chain reaction; in the U^{238}-Pu^{239} cycle, by contrast, the breeding process requires that fast neutrons be used. The main technological approaches to breeding have therefore gone along two courses: thermal-neutron Th-U^{233} reactors and fast-neutron U-Pu^{239} reactors.

The theoretical margin for breeding can be increased by using certain auxiliary neutron reactions which increase the neutron yield without destroying U^{233} or Pu^{239}. Thus U^{238} itself can be made to fission and to produce neutrons if the energy of the incident neutron exceeds 1.0 Mev. The gain in breeding ratio achievable by mixing U^{238} intimately with the fissile material in a fast neutron reactor is considerable - perhaps as much as 0.3. Rather analogously some light elements, notably Be, undergo so-called (n,2n) reactions with fast neutrons, according to the equation

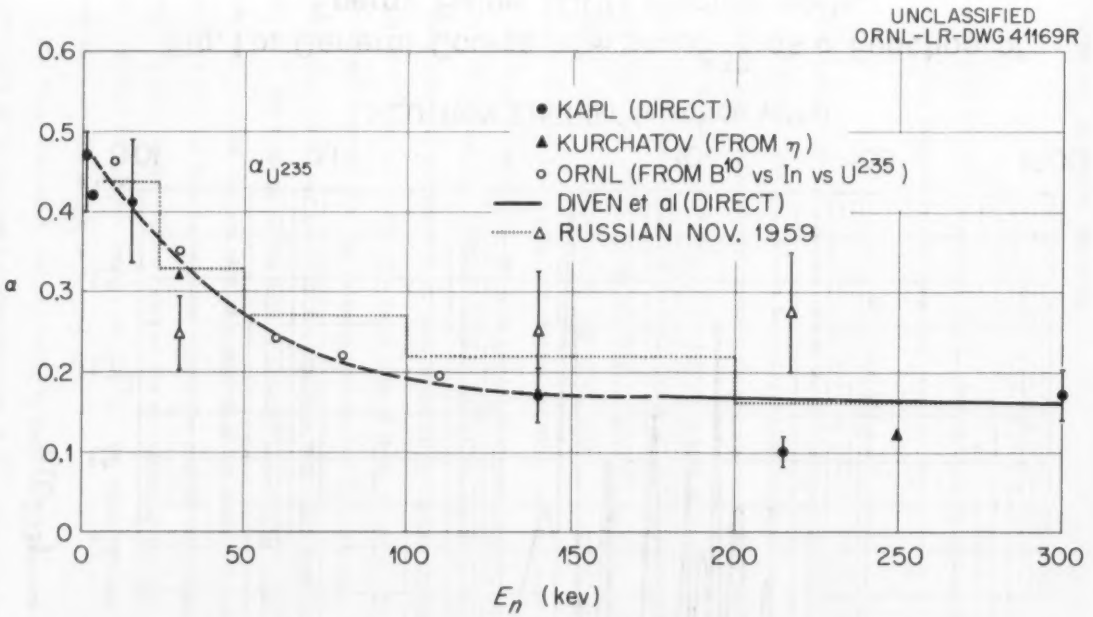

Fig. 1a. Variation of the Ratio of Capture to Fission in U^{235} as a Function of the Energy of the Neutrons Producing the Fission. The Value of η is $2.5/(1+\alpha)$ and is Therefore Considerably Higher at 300 kilovolts than at 10 kilovolts. Similar Behavior is Found for all the Fissionable Nuclides Including U^{233} and Pu^{239}.

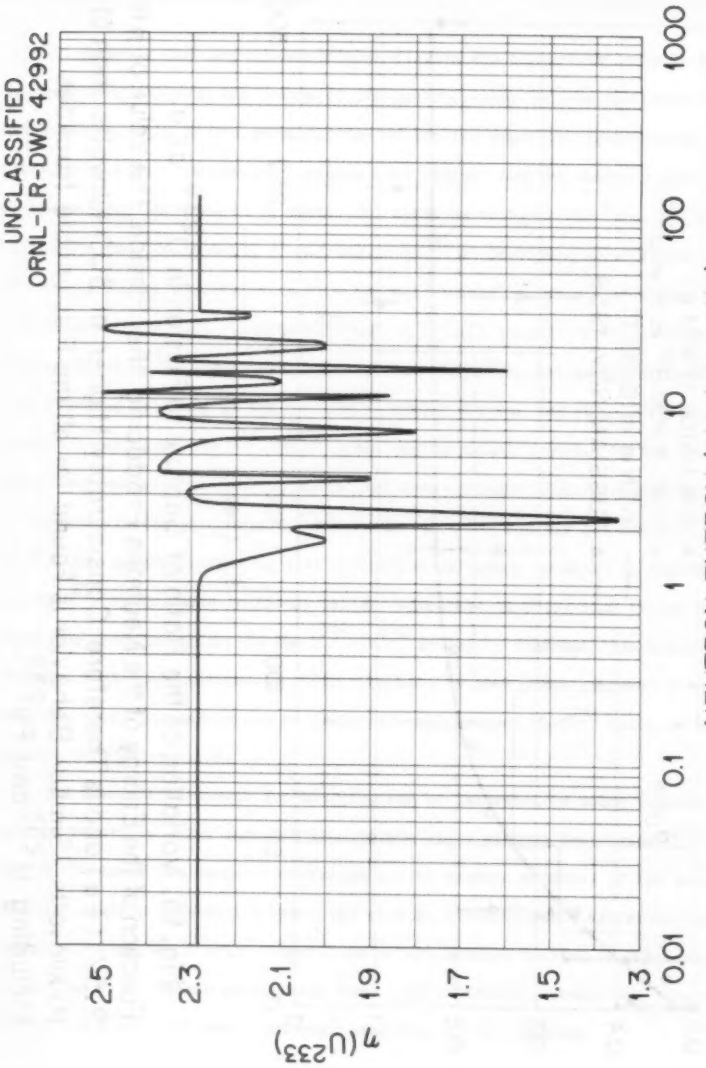

Fig. 1b. General Course of η of U^{233} as a Function of Energy Below 1000 electron volts.

$$Be^9 + n \rightarrow 2\ He^4 + 2\ n \qquad (3)$$

which also increase the neutron yield. However, in the case of Be the (n,2n) reaction is somewhat balanced by the competing reaction

$$Be^9 + n \rightarrow Li^6 + \alpha \qquad (4)$$

which loses a neutron and more, since the Li^6 produced in this reaction is an avid neutron absorber. The net balance is probably slightly favorable, and Be-moderated thermal breeders are considered with favor by many groups throughout the world.

The margin for breeding in the thermal system is so small that a great deal of work has gone into measurement and re-measurement of $\eta(U^{233})$ for thermal fission. As a result of all this effort, which has extended over the last 15 years, the η of U^{233} for thermal neutrons is now probably one of the most accurately known nuclear constants - as well it might be, since, in a degree, the whole energy future of mankind may be tied to the value of $\eta(U^{233})$. The most precise measurements have been those performed recently in two entirely different experiments at the Oak Ridge National Laboratory. The first, Figure 2, performed by R. Macklin and G. deSaussure consisted of placing a small foil of U^{233} in the center of a large bath of $MnSO_4$ solution. The foil was irradiated by thermal neutrons from the Swimming Pool Reactor at Oak Ridge. All the neutrons accompanying fission are caught in the manganese bath where they induce radioactivity in the Mn^{55}. The activity of the bath is measured essentially twice: once with the foil removed, in which case one obtains a measure of the number of neutrons in the beam; and once when the foil is in place. Since the foil is thick enough to catch all the neutrons which strike it, the ratio of the activities of the bath with and without the foil in place is a direct measure of $\eta(U^{233})$. The value obtained by Macklin and deSaussure by this method is $2.295 \pm .015$.

The other experiment, carried out by R. Gwin and D. Magnuson at Oak Ridge (Figure 3) consists of determining the concentration of U^{233} which must be dissolved in an in-

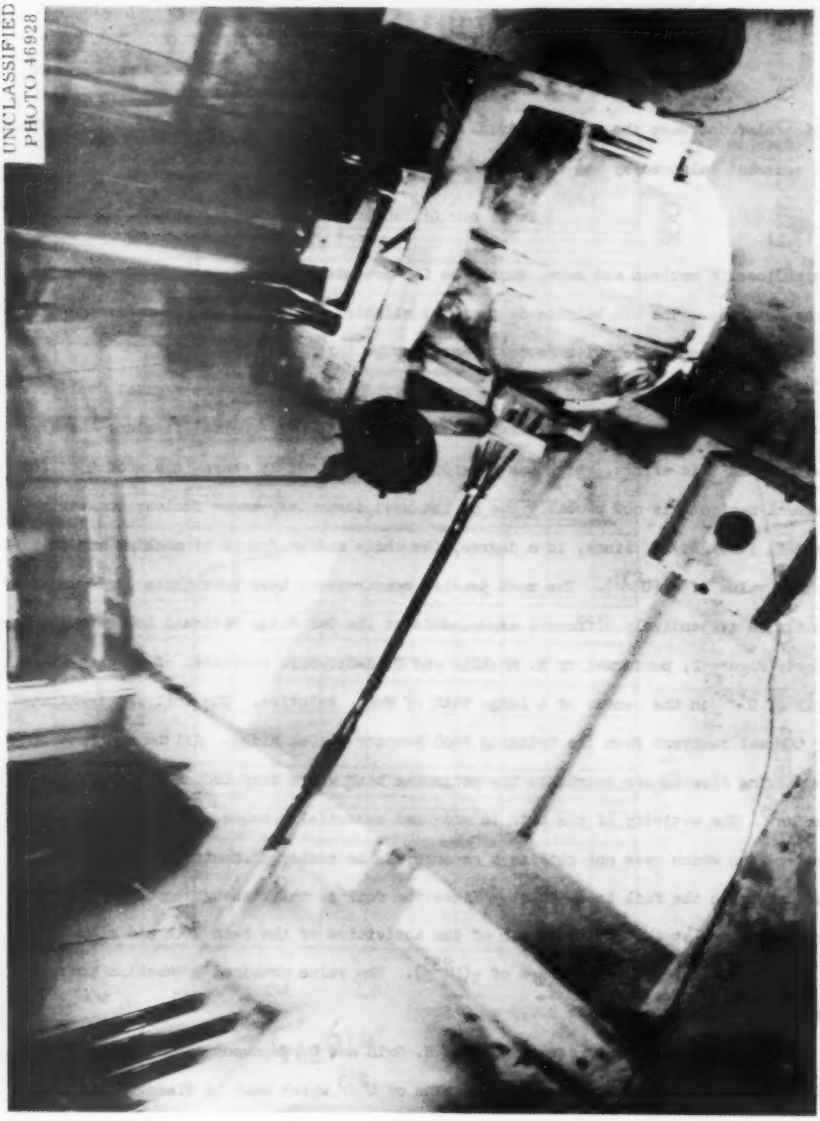

FIGURE 2a.

FIGURE 2a.

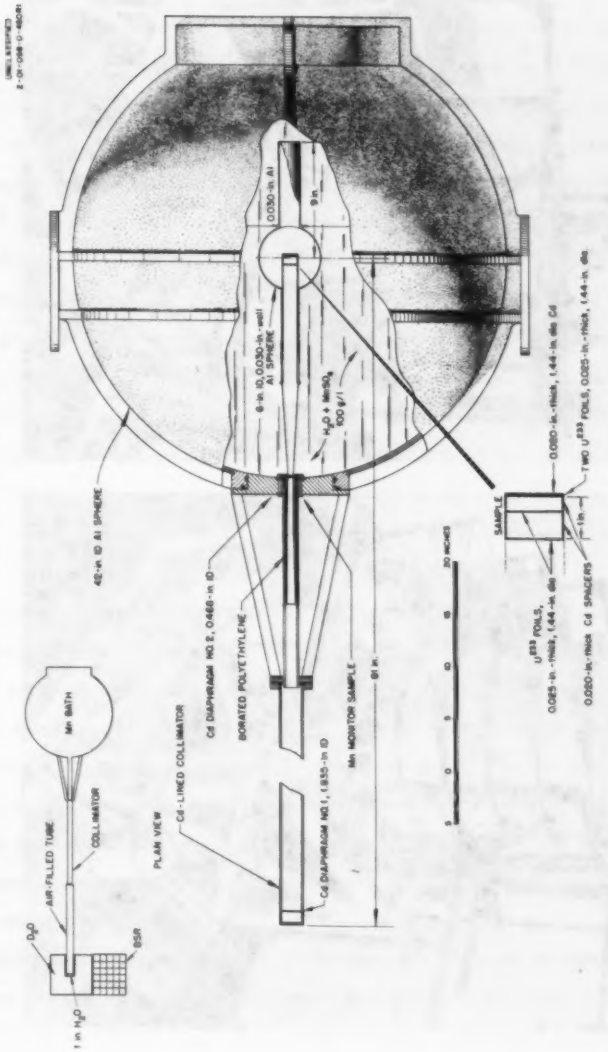

Figure 2b. Experimental Arrangement for Measurement of Eta of U^{233}.

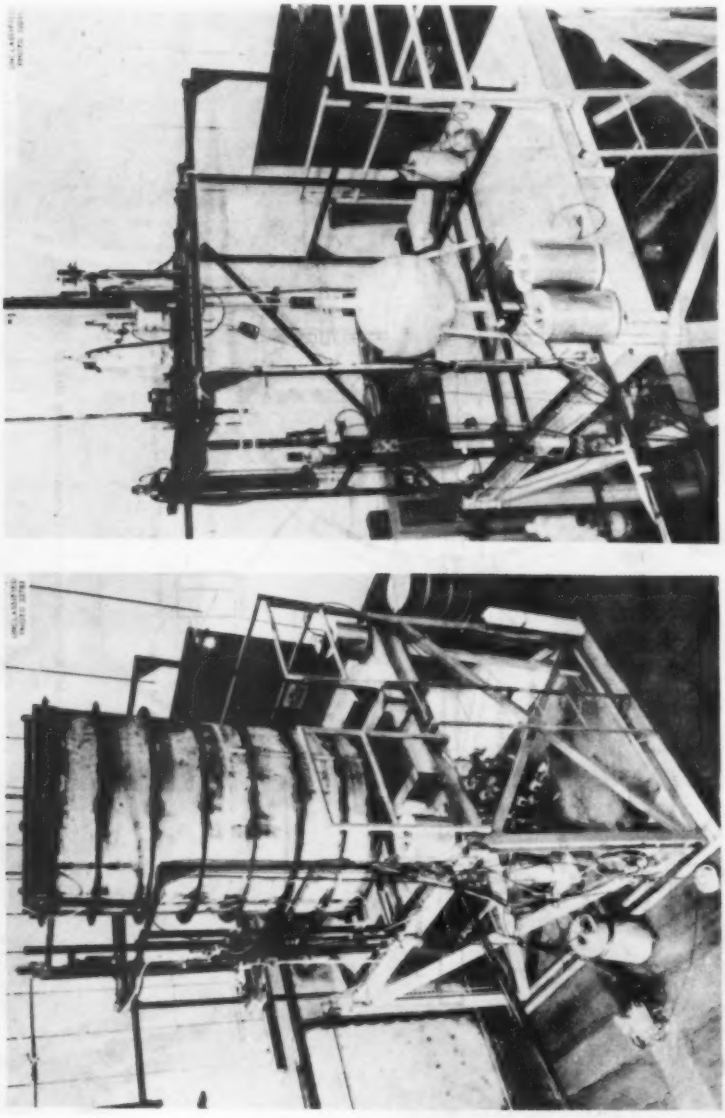

Fig. 3. Two Different Sizes of Critical Tanks used in the Ocean Experiment.

finitely large bath of pure H_2O in order to make the resulting solution chain-reacting. Of course it is not possible to construct an infinitely large tank; instead a series of increasingly larger tanks of water were made critical by adding U^{233} nitrate to each one until the chain reaction began; the concentration of U^{233} in an infinitely large system was estimated by extrapolation. Since the largest tank of solution used in this experiment was relatively so large (5-foot diameter cylinder containing 32 kgs of U^{233} compared to about 18-inch diameter and 3 kgs of U^{233} which is the minimum-sized critical U^{233}-H_2O chain reactor) the experiment is referred to as the "Ocean" experiment. In a just-critical ocean of U^{233} and H_2O, the net increase in number of neutrons produced per neutron absorbed in U^{233} - that is $\eta - 1$ - is just balanced by the absorption in the other constituents of the water - hydrogen and oxygen. Since the probability of thermal neutron absorption in hydrogen is the best of all known constants of nuclear dynamics ($\pm$ .3%) and the probability of absorption in U^{233} is only slightly less well-known ($\pm$ 1%), it was possible to determine $\eta - 1$ to about 1%. The result of this ocean experiment is $\eta = 2.28 \pm .02$ in gratifying agreement with the results of the manganese bath experiment.

The net result of a great deal of theoretical and design engineering work, based on a value of $\eta(U^{233}) = 2.28$, is that in a thermal breeder using the Th-U^{233} cycle a practical breeding ratio, counting all losses, of about 1.10, should be achievable; and in a fast breeder, based on the U-Pu^{239} cycle, a practical breeding ratio of approximately 1.6 should be achievable. The breeding ratio in the thermal system is considerably lower than in the fast system; however, the specific power in the thermal system ought to be much higher than in the fast system, and there is good ground for the present belief that doubling times of the order of 10 to 20 years ought to be achievable in either system.

Why should the specific power in the fast Pu^{239} breeder be significantly lower than in the thermal breeder? The reason, basically, is that the fuel in the fast

breeder must not be diluted very much by mixing with too much coolant or structural material - otherwise, collisions with the foreign atoms will degrade the energy of the fast neutrons emitted in fission and $\eta(Pu^{239})$, which decreases sharply as the neutron energy falls, may not be high enough to make breeding possible. But in order to extract heat efficiently the uranium fuel ought to be as finely divided and therefore diluted as much as possible. Thus the fast breeder is faced basically with the dilemma of "unusable compactness" versus "unworkable diluteness". The thermal breeders by contrast are intrinsically more dilute systems. The fissile material is always mixed, in varying degrees of intimacy, with large amounts of moderator such as D_2O or beryllium; heat removal is a fundamentally easier job in a thermal reactor than in a fast reactor.

To illustrate the point, the specific power in the Enrico Fermi Fast Breeder Reactor now being built near Detroit is about 200 kilowatts of heat per kilogram of fissile material; by contrast, the specific power in the Homogeneous Reactor Experiment No. 2 which is a partial prototype of a U^{233} thermal breeder, is about 700 kw heat per kilogram, and in a full-scale homogeneous breeder reactor the estimated specific power is about 2000 to 3000 kw per kilogram. Of course the specific powers in both fast and slow reactors would be reduced somewhat if the fissile material held up in the chemical reprocessing plants is counted; nevertheless, the thermal system ought to be about five times better than the fast system in specific power, and this advantage just about balances the disadvantage in breeding ratio.

Engineering Considerations

A breeder reactor usually consists of a "core" of fissile material - U^{233} or Pu^{239} - surrounded by a "blanket" of fertile thorium or uranium. The bulk of the energy is produced in the core, and the neutrons which leave the core are caught in the blanket where they convert fertile material into fissile material.

In fast breeder reactors there is advantage to mixing some fertile U^{238} with the fissile Pu^{239} in the core since, as we have pointed out, fast neutron fission in U^{238}

adds significantly to the breeding ratio. In addition, dilution of the Pu^{239} with U^{238} improves the heat transfer characteristics of the reactor, and all designs for fast breeder reactors show U^{238} in the core as well as in the blanket. In thermal breeder reactors based on the thorium cycle, there is strong incentive to keep the fissile and fertile materials separate; any gain in breeding ratio which results from fast fission in Th^{232} mixed intimately with U^{233} would be much more than lost by strong neutron absorption by the intermediate nuclide Pa^{233} which is formed when Th^{232} absorbs a neutron, and which decays with 27-day half-life to U^{233}. In the core of a thermal breeder the neutron flux may be so high (10^{15} neutrons/cm^2/sec), that there is a good chance for Pa^{233} to be converted into the useless Pa^{234} and then to U^{234} by neutron absorption before the Pa^{233} has decayed to the useful U^{233}. In the blanket, which is relatively farther removed from the seat of the chain reaction, the neutron flux is much lower, and almost none of the Pa^{233} is transmuted into U^{234} before decaying to U^{233}. Hence all the fertile material is always confined to the blanket of a thermal breeder rather than being put in both core and blanket as in a fast breeder.

The basic feasibility of breeder reactors, both thermal and fast, is no longer in doubt. The development of their technology has followed two quite distinct conceptual strategies. The most straightforward, or "hard-headed" strategy is to start with an existing, non-breeder reactor technology - say the technology of gas-cooled reactors, such as the British Calder Hall reactors - and to improve the technology by increments - saving a tenth of a neutron here and a tenth of a neutron there - until the basic type has been converted into a reactor which breeds. The other strategy is, by contrast, rather "egg-headed"; it is to develop a full-blown technology specifically for reactors which are embodiments of the physicists' conception of how a breeder should look. The two strategies tend to overlap; of the three major efforts toward developing breeder reactors, two, aimed at the fast Pu breeder and at the thermal, liquid-fuel U^{233} breeder, may be described as employing the egg-headed strategy; the third, based on gas-cooled graphite- or beryllium-moderated solid fuel systems, employs the hard-headed strategy.

THE PROBLEMS OF BURNING THE ROCKS

Fast Neutron Pu Breeding

The largest share of effort throughout the world on breeder reactors has been devoted to the fast Pu^{239} breeder. This is natural since the breeding ratio is so large in this system that there is no question that fast neutron breeding is possible. Major projects on fast breeders are underway in the United States, United Kingdom, and Soviet Union, and there has been serious consideration of this reactor type in almost every country with a reactor development program.

The first fast breeder type reactor was the Experimental Breeder Reactor No. 1 (EBR-1) which was designed and built by a group under W. H. Zinn, then Director of Argonne National Laboratory, at the National Reactor Test Station in Idaho. This reactor, which is still operating, is rated at 1400 kw of heat, and was the first reactor to generate electrical energy (December 20, 1951). The reactor core consists of fuel rods of nearly pure U^{235} jacketed in stainless steel; the diameter of the rods is about 0.3 inches, and the entire core is as large as a "football". The blanket consists of 1-inch-diameter, natural-uranium rods clad in stainless steel. The reactor is cooled by liquid sodium, the exit temperature of which is 316 °C. The original fuel of EBR-1 was U^{235} rather than Pu^{239}, since the U^{235} is generally much easier to handle than is Pu^{239}. Since η for fast neutron fission in U^{235} is only about 2.4, compared to 2.7 for Pu^{239}, the breeding ratio measured in EBR-1 was very close to unity - $1.0 \pm .05$. The specific power was also very low - only about 18 kw per kg of U^{235}. Nevertheless, the demonstration that a fast neutron reactor which resembled a Pu breeder could be operated reliably at significant temperature and power, and that a breeding ratio of about one could be achieved, was a major milestone in nuclear energy development. The EBR-1 did suffer an accidental melt-down several years ago during experiments designed to test the reactor's transient behavior; however, the reactor has since been rebuilt and has operated routinely since 1957.

THE PROBLEM OF BURNING THE ROCKS

Encouraged by the success of EBR-1, several groups have proceeded to construct much larger fast reactors. In the United States two large fast reactors are under construction - EBR-2, rated at 60 Mw heat, 15 Mw electric, being built by Argonne National Laboratory at Arco, Idaho, and the Enrico Fermi Reactor, rated at 300 Mw heat, 100 Mw electric, being built by Atomic Power Development Associates at Laguna Beach near Detroit. In U. K., a fast reactor similar to EBR-2 has been completed at Dounreay, Scotland, and is now undergoing initial trials. In the U. S. S. R. a whole sequence of increasingly larger fast reactors is being planned; of these, two, with outputs of 100 and 5000 kw of heat, have been built.

All of these reactors are basically similar and I shall therefore describe only the Enrico Fermi Reactor (Figure 4). In all cases the reactors are cooled with liquid sodium; and the initial fuel of the Western reactors is U^{235} rather than Pu^{239}, the intention being to switch to Pu when the reactors have been shaken down. (The 5000 kw fast reactor in the U. S. S. R. uses PuO_2 as fuel.) The Fermi Reactor, an extraordinary engineering tour de force, illustrates the basic engineering difficulty of fast reactors: the core, in which most of the 300 Mw of heat is produced, is a cylinder only about 1-meter high and 1 meter in diameter - a total volume of about 600 liters. Thus, per unit of core volume, the reactor will produce 500 watts per cubic centimeter. On the other hand, the power per kilogram of fuel, or per ton of fertile material, is low. The fuel consists of an alloy containing three atoms of U^{238} to one atom of U^{235}; 10 weight per cent of molybdenum is added to render the alloy less susceptible to radiation damage. This molybdenum-uranium alloy is fashioned into zirconium-clad fuel pins, about 0.16 inch in outside diameter, which are stacked side by side with room for flow of the sodium coolant. The total U^{235} inventory is 1450 kgs, making a specific power of only 200 kw per kg; the inventory of U^{238} in blanket and core is ~~250~~ 40 tons.

The reactor operates submerged in a pool of sodium which serves as a plenum for the coolant sodium. The sodium leaves the reactor at about 400 °C and produces 600 pounds per square inch steam in especially designed sodium-steam generators. The entire

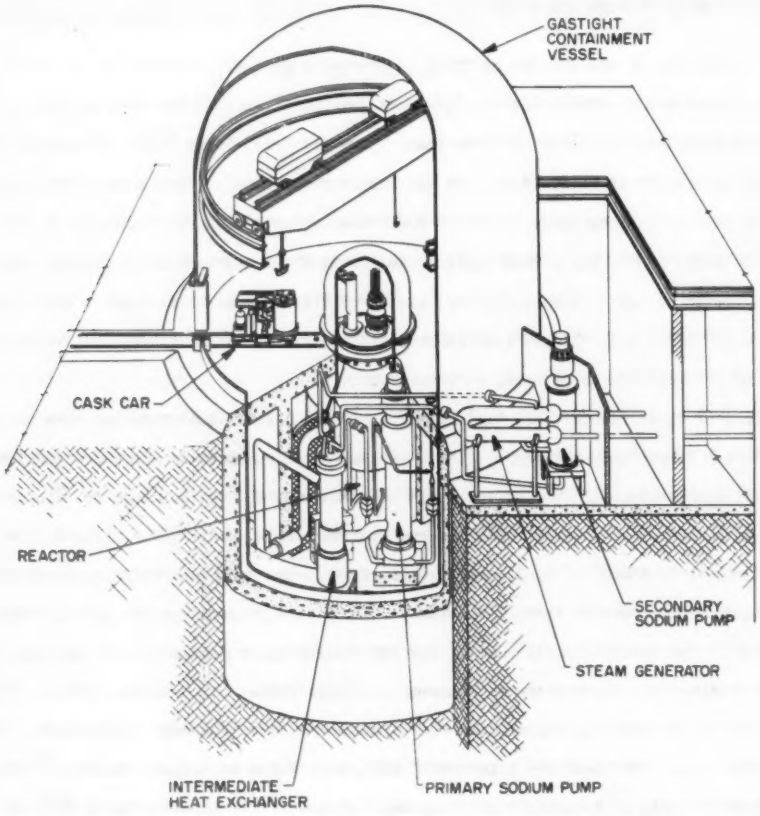

FIGURE 4. PERSPECTIVE VIEW OF ENRICO FERMI NUCLEAR POWER PLANT.

reactor complex is housed in a large spherical containment vessel which is 120 feet high, has an inside diameter of 72 feet, and is made of 1-inch thick steel.

There are two essential economic problems in the fast breeder. The first is the problem of burnup of the fuel; the second, the problem of holdup of fissile and fertile material. As for the first, burnup of fuel, it is expected that only about 10% of the U^{235} in the reactor will be consumed before the fuel elements will be so damaged by radiation as to require reprocessing. Thus, in order to burn one gram of U^{235} in the reactor, 10 grams of U^{235} must be reprocessed. If each reprocessing costs $5 per gram, then the effective cost of the fuel is about $50 per gram plus the initial cost of U^{235} - a total of, say, $65 per gram. This leads to a fuel cost of about 13 mills per kwh which is perhaps four times the energy cost of conventional fuel. The sale of bred plutonium, it is true, will reduce this cost some, but hardly enough to make fast breeding economical unless fuel elements which withstand much longer burnup are developed, or the cost of reprocessing is reduced, or both. Important progress in both these directions is being made at Argonne's EBR-2 - by use of "fissium", an alloy of uranium and rare earths found in the fission products, which seems to be less susceptible to radiation damage than is U-Mo alloy; by greatly simplifying reprocessing and refabricating techniques - in particular, by casting rather than machining the fuel elements; and by using the radiation-resistant PuO_2, as in the U. S. S. R. reactor, rather than metallic fuel elements.

The other economic difficulty - high holdup of fertile material - is manifested only if the cost of U^{238} goes a good deal higher than its present $10 per pound. At present prices, assuming a total inventory in reactor and chemical plant of about one-half ton per Mwe, the inventory cost is only about $10 per kw. On the other hand, if uranium costs eventually increase by a factor of 50, the inventory cost would become $500 per kw, although the burnup cost still remains negligible - about 0.2 mills per kwh. Such an inventory cost would be prohibitively high for reactors. We shall see

presently that there is a neat way around this dilemma even if the technology does not develop reactors with greatly reduced fertile material inventories.

Thermal Neutron, U^{233} Breeding

The central problem of the fast breeder - achievement of long fuel burnup without reprocessing - can be circumvented in the thermal breeder by putting the reactor fuel into solution. If the fuel is in liquid form, radiation damage no longer imposes a limitation on the amount of burnup achievable before reprocessing is required; instead, the limitation is imposed by the much less stringent requirement of keeping the concentration of fission products (which absorb neutrons) at an acceptable level. This is one of the basic rationales for the aqueous homogeneous thorium breeder, to a description of which we now turn.

To date two high-powered homogeneous reactors, Homogeneous Reactor Experiments 1 and 2 (HRE-1 and HRE-2), aimed specifically at developing a technology for thorium breeders have been operated at ORNL; and three other high-power liquid fuel reactors aimed not so directly at the breeding objective have been operated for short periods - the two Los Alamos high-pressure uranyl phosphate reactors (LAPRE-1 and LAPRE-2) and the fusted salt Aircraft Reactor Experiment (ARE) at ORNL. I shall describe HRE-2, which was intended originally to be a partial prototype of a thorium breeder reactor.

The HRE-2 consists of two regions or zones (Figure 5). The inner region is a 32-inch-diameter pear-shaped tank made of a zirconium alloy; through this tank is pumped a solution of enriched uranyl sulfate - $U^{235}O_2SO_4$ - in heavy water. The tank is of a shape which allows a critical mass to accumulate there; the resulting chain reaction produces heat which is carried away by the circulating, radioactive fluid.

Surrounding the zirconium tank is a stout steel pressure vessel of 4-foot diameter. Heavy water and, ultimately, a slurry of ThO_2 in heavy water were to be pumped through the region between pressure vessel and core tank, the slurry constituting a blanket in which U^{233} was to be produced. However, in the course of early power runs at HRE-2,

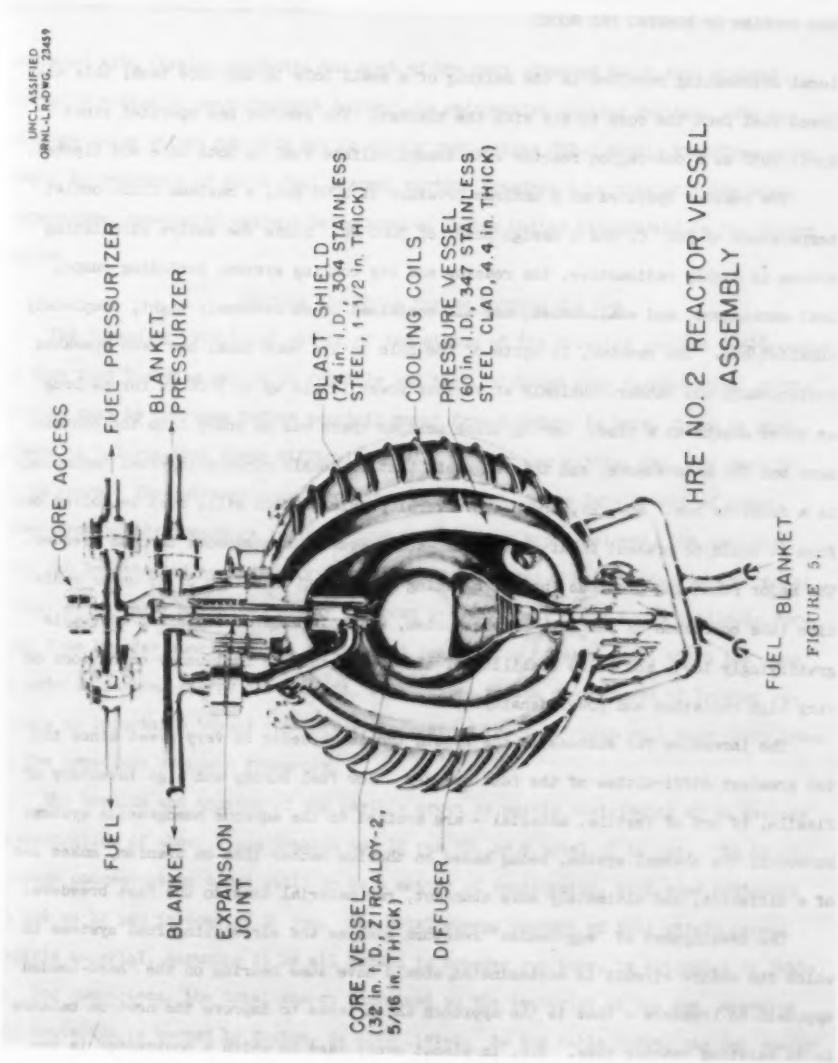
UNCLASSIFIED
ORNL-LR-DWG. 23459
CORE ACCESS
FUEL PRESSURIZER
BLANKET PRESSURIZER
BLAST SHIELD (74 in. I.D., 304 STAINLESS STEEL, 1-1/2 in. THICK)
COOLING COILS
PRESSURE VESSEL (60 in. I.D., 347 STAINLESS STEEL CLAD, 4.4 in. THICK)
HRE NO. 2 REACTOR VESSEL ASSEMBLY
FUEL
BLANKET
EXPANSION JOINT
CORE VESSEL (32 in. I.D., ZIRCALOY-2, 5/16 in. THICK)
DIFFUSER
FUEL BLANKET

FIGURE 5.

THE PROBLEM OF BURNING THE ROCKS

local overheating resulted in the melting of a small hole in the core tank; this allowed fuel from the core to mix with the blanket. The reactor has operated since April 1958 as a one-region reactor with uranyl sulfate fuel in both core and blanket.

The reactor operates at a maximum pressure of 2000 psi, a maximum fluid outlet temperature of 300 °C, and a design power of 5000 kw. Since the entire circulating system is highly radioactive, the reactor and its cooling system, including pumps, heat exchangers, and auxiliaries, are all contained in an extremely tight, completely shielded steel box. The reactor, in spite of the hole in its core tank, has been operated continuously and rather routinely at various power levels up to 5000 kw for as long as three months at a time. During these periods there was no entry into the containment box for maintenance, and this suggests that the basic circulating-fuel technology is a feasible one. However, there are several problems which still must be solved before it would be prudent to attempt a full-scale aqueous homogeneous thorium breeder. The major remaining problems include handling of slurry, corrosion of Zr under radiation (the corrosion of the bulk of the system, which is made of stainless steel, is gratifyingly low), and phase stability of the uranyl-sulfate fuel under conditions of very high radiation and power density.

The incentive for success in the liquid thorium breeder is very great since the two greatest difficulties of the fast breeder - low fuel burnup and high inventory of fissile, if not of fertile, material - are avoided in the aqueous homogeneous system. Moreover, the thermal system, being based on thorium rather than on uranium, makes use of a different, and ultimately more abundant, raw material than do the fast breeders.

The development of "egg-headed" reactors, such as the circulating-fuel systems in which the entire circuit is contaminated, should have some bearing on the "hard-headed" approach to breeders - that is the approach which tries to improve the neutron balance in an existing reactor type. For, in almost every case in which a nonbreeder is redesigned into a breeder, the first step is to somehow get rid of the neutron-absorbing cladding on the fuel elements. Absence of cladding causes the coolant to become con-

taminated with fission products, and most of the very advanced solid fuel element thermal breeders or near-breeders contemplate radioactive cooling systems. The experience being gained now with the intensely radioactive HRE-2 should therefore prove useful to designers of solid fuel thermal neutron breeders - particularly the high-temperature, gas-cooled systems being studied in the United Kingdom and in the United States.

Burning the Rocks versus Burning the Sea

The foregoing very brief sketch of the status of the breeding problem was intended to show that breeding per se is feasible and that, although some technological difficulties must be overcome before economic power from breeders is here, there is good reason to believe that these difficulties will be overcome within, say, the next 10 to 20 years. The extraordinary importance of success in the development of cheap power from breeder reactors is that, once such systems are developed, the fuel for them can be obtained economically from the residual uranium and thorium in the rocks; hence, essentially unlimited energy at about present costs ought to be available forever from breeder reactors. It is the full realization of this fact, which has come so late in nuclear energy development, which gives to the development of breeder reactors an importance beyond that of development of reactors which will make cheap power in the immediate economic framework.

The uranium and thorium of the earth's crust is mostly distributed at an average concentration of about 4 ppm uranium and 12 ppm Th, or a total of 16 ppm. The exact average concentration seems still to be a matter of controversy, with some estimates as low as 12 ppm instead of 16 ppm. The total energy content of this widely-spread fertile material, assuming it is all burned in breeder reactors, is estimated in Table 1. For comparison, the total energy contained in the deuterium of the sea, assuming the deuterium is burned by fusion, is also listed. As the table shows, the two energy contents are of the same order; if anything, there seems to be more energy in the fissionable materials than in the fusionable. Just how long would this energy supply last?

THE PROBLEM OF BURNING THE ROCKS

This of course depends on the rate at which it is used. If we assume, with Harrison Brown, that the world will finally settle down to an energy consumption of 4×10^7 Mw (this is 20 times the present energy input), then the energy reserve in either fission or fusion would last for more than 10^{10} years.

Table 1

Amounts of Deuterium, Li^6, U, and Th on Earth*

Raw Material	Source	ppm	Total Mass (Tons)	Energy Content (Megawatt Days of Heat)	Will Last at 4×10^7 Mw (Heat) Rate
Deuterium	Sea	33	6×10^{13}	2.4×10^{20}	1.7×10^{10} yrs.
Li^6	Crust	5	10×10^{13}	4×10^{20}	2.7×10^{10}
U	Crust	4	8×10^{13}	$.75 \times 10^{20}$	0.5×10^{10}
Th	Crust	12	25×10^{13}	2.3×10^{20}	1.5×10^{10}
Total U + Th	Crust	16	33×10^{13}	3×10^{20}	2×10^{10}

*The basic data in this table are from B. Mason, "Principles of Geochemistry", p. 41, John Wiley and Sons, New York (1952).

In the case of fusion it is clear that the raw material is relatively easy to recover from nature. Thus the present cost of deuterium is 30¢ per gram; the burning of one gram of deuterium in a fusion reactor at 25 per cent thermal efficiency yields about 24,000 kwh of electricity. The fuel burnup charge therefore amounts to about 0.015 mills per kwh which is negligible, and which, moreover, should always remain negligible since the concentration of deuterium in the sea will remain at its present value for aeons to come.

At first sight the situation is not so clear in the case of the fissionable materials. Energetically the balance is certainly favorable. Brown has estimated that the energy required to extract about one-fifth of the U + Th - i.e., about three grams of U + Th - from one ton of granites is between 25 and 48 pounds of coal equivalent. This energy is divided as shown in Table 2.

Table 2

Energy Required to Recover Residual U + Th from Granite

Quarrying, crushing, hauling	7-9 kwh/ton
Acids	10-25 kwh/ton
Other direct charges (water pumping, etc.)	1 kwh/ton
Hidden costs	0.7-2.3 kwh/ton
	19-37 kwh/ton or 25-48 lbs. coal

The energy recovered from the burning of three grams of fissionable material is about 10 tons' coal equivalent. Thus about 400 times as much energy is retrieved from "burning the rocks" as is required to mine and process them, assuming of course that the fertile U^{238} and Th^{232} rather than merely the U^{235} are burned.

Cost estimates for extracting this low-grade material are quite difficult to come by. Brown estimates that a ton of granite could be processed for $1.00 to $2.25, which would lead to an asymptotic cost of fertile material of 30¢ to 80¢ per gram; other estimates suggest that $1.00 per gram is more realistic. Even at $1.00 per gram, the burnup cost is only 0.18 mills per kwh, which, while not quite negligible, is still a very small fraction of the total cost of generating a kilowatt-hour of electricity.

There remain four other questions to settle before we can conclude that rock burning can be made an economical, ultimate, energy source.

First, nowhere near all of the earth's crust is accessible for mining. We ought not to count parts of the crust under the sea, nor the material more than 3 km below the surface, nor the granites which carry a great overburden of sediments which are poor in fertile material. On the other hand, since the energy balance is so favorable, one could mine rocks for as little as 0.3 grams per ton of uranium and still get about 20 times more energy than is required to extract the fissionable material; an increase by even a factor of 10 in the cost per gram - to $10 per gram - would still leave the burnup cost at 1.8 mills per kwh which is comparable to the present cost of

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THE PROBLEM OF BURNING THE ROCKS

coal. One therefore cannot escape the impression that the extractable reserve of fertile material is large enough to sustain humanity indefinitely.

Second, we must estimate just how large a mining operation would be required to allow us to support our entire energy system by "burning the rocks". Again assuming an asymptotic requirement of 4×10^7 Mw heat - i.e., 20 times our present system - about 40 tons of fertile material per day are required. To obtain this amount of material would require the mining of about 10^7 tons of rock per day, which is to be compared with the world's daily production of coal and lignite in 1953 of about 0.6×10^7 tons. Thus the whole mining operation, to support an energy economy 20 times larger than our present one, would hardly be larger than the present coal mining operation.

Third, there is the matter of paying for the initial inventory of fertile material in our breeders. At the previously-mentioned rating of one-half ton of fertile material per megawatt of electricity, the inventory cost at $1 per gram of fertile material is $500 per kw - a very high charge indeed, though lower than the capital cost of $1000-$2000 per kw estimated for solar energy. Undoubtedly future developments will considerably reduce the inventory per kilowatt. However, even without such improvement, the high inventory cost can be circumvented by using cheap uranium and thorium from high-grade sources for inventory, and then keeping the reactors going, ad infinitum, with U + Th from low-grade sources. Thus one can foresee a situation in the future where cheap uranium, tied up as inventory in nonbreeding reactors, is transferred to breeder reactors at such time as the cost of uranium becomes so high as to make it uneconomical to continue to burn only a small fraction of the uranium in the non-breeders.

Finally, there is the question of handling the huge amounts of radioactive wastes which will be produced as a by-product of such a fission energy system. This is perhaps the least clear part of the rock-burning story. What can be said is that serious

effort aimed at developing ultimate methods of waste disposal is now going on at many atomic energy sites. The ultimate methods involve using the earth itself, whence the unused fissionable material came, as the grave for the radioactive wastes. Disposal in salt domes, in deep natural wells, and in artificially-created crustal fractures - each has its proponents. It is premature to say that any system now being investigated will prove to be entirely satisfactory; it is gratifying that a recent ad hoc committee of the American Petroleum Institute, which was set up to examine ultimate geologic methods of waste disposal, concluded that several of the proposed deep well and salt disposal schemes were feasible.

Other Possible Ways of Burning the Rocks

It is most remarkable that in the process of nuclear breeding we have found a way to forestall the catastrophe which our predilection for human breeding has always seemed to make inevitable. That we see a practical way, essentially with present-day technology, of producing energy for all time at costs very close to present costs, is to my mind little short of miraculous. Before closing, however, it will be well to mention a few of the conceivable alternate schemes for burning the rocks, other than by breeder reactors, which have received attention.

A variant of the fast breeder is the nuclear explosion underground, with the extraction of heat and bred fuel from the hot magma created in the explosion. The main advantage of such a scheme is that the expensive reactor is replaced by an inexpensive bomb, and that much of the energy can be obtained from fusion of relatively cheap light elements rather than from fission of expensive Pu^{239}. The main disadvantage is that the scavenging of the rock for newly-bred Pu^{239} involves a very difficult large-scale chemical extraction. Whether in practice, taking all losses into account, a breeding ratio greater than one can really be achieved with the bomb-breeder remains somewhat obscure. Moreover, if, as we assume, the ultimate energy system requires energy equivalent to the fission of 40 tons of fissionable material per day, this would amount to

THE PROBLEM OF BURNING THE ROCKS

about 6800 megatons of TNT equivalent set off in nuclear bombs every day. Granted that these would be set off at great depth, and that much of the energy comes from the relatively clean fusion process instead of from fission, it is still hard to see how so vast a flood of rather uncontrolled radioactivity could ever become practical.

Another possibility which, in fact, was the basis for a large project under the direction of E. O. Lawrence from 1950 to 1953 is the so-called electrical breeder. The electrical breeder is based on the observation that protons or deuterons of, say, 400 Mev will produce, on striking a heavy target such as U^{238}, Th^{232}, or Bi, as many as 20 neutrons per incident particle. These neutrons can be caught in a blanket of U^{238} or Th^{232} which might surround the original target. The resulting 20 atoms of Pu together with their progeny when burned in a near-breeding reactor could provide a handy net energy gain. The full practicality of electrical breeding is hard to evaluate; certainly the electrical gear, huge linear accelerators, and enormous targets which are needed in addition to the Pu-burning reactors, make it hard to see how this system can compete well with breeder reactors alone. However, Lawrence's scheme is an intriguing one, and I am sure that variants of it will be suggested from time to time.

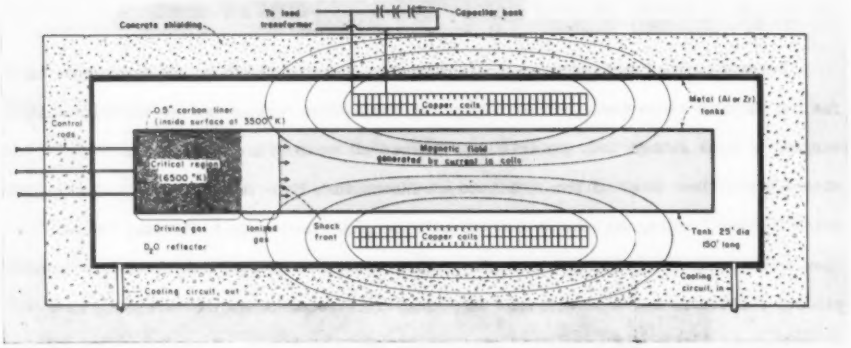
All of the proposed long-term large-scale energy production systems - the various rock-burning schemes and controlled fusion - will obviously be bought by mankind only if they are economically superior in the long run to large-scale solar energy. To make economic comparisons between solar and nuclear energy is obviously extremely difficult. What can be said is that solar energy is estimated to require a capital expenditure of between $1000 and $2000 per kw; no alternate energy system can be expected to compete unless it entails a capital cost significantly less than, say, $1500 per kw. In this respect breeder reactors ought to be very good; their capital cost ought to be no more than about $200 to $300 per kw provided they are inventoried with cheap uranium and thorium, and are merely kept going with the expensive residual material in the rocks.

THE PROBLEM OF BURNING THE ROCKS

The solution of mankind's long-term energy problem by breeding, as contrasted to fusion, is in a sense inelegant - it requires mostly the working out of tedious engineering details rather than any brilliant flashes of scientific insight. As Academician Bogolyubov, Head of the Institute of Theoretical Physics at the U. S. S. R. Joint Nuclear Center, said to me recently, "Breeding constitutes a 'trivial' solution to man's ultimate energy problem". Perhaps it is because of its essential simplicity implied in the word trivial that those interested in nuclear breeding find it such an alluring, and hopeful, technological development.

(Material later submitted dealing with this subject follows:)

10/27/59



Plasma Reactor Promises

A proposed reactor converts fission energy directly into electricity by causing ionized U^{233} gas to interact with a magnetic field. Estimated operating conditions look extreme but not impossible

By S. A. COLGATE *Radiation Laboratory, University of California, Livermore, California*
and R. L. AAMODT *Los Alamos Scientific Laboratory, University of California, Los Alamos, New Mexico*

THE PRESENT DAY procedure for converting nuclear energy into electric power must have at some time stirred rebellion in the heart of every designer of power reactors. Besides being unaesthetic and unimaginative the conventional arrangement of intermediate coolant circuits, heat exchangers and turbogenerators is also cumbersome, inefficient and expensive to build and maintain. No doubt an impressive number of man-hours have already been spent by people in the field daydreaming about schemes for converting nuclear energy directly into electricity. Some have gone past the imagination stage; the atomic battery, a device that uses the energetic ions from a radioactive source to charge up a small capacitor, is an example of a successful application. So far, however, no one has advanced an economically practical scheme for making electricity directly from the fission power produced in a nuclear reactor.

With the hope of alleviating this situation we have been rash enough to put down one of our own personal daydreams on paper. Surprisingly enough our rough design calculations show that the scheme may prove to be technically feasible and possibly even economically feasible. We offer, then, for further consideration the conceptual design of a reactor that converts the energy produced in the fissionable material directly into electrical energy.

The proposed reactor is cylindrical in shape and would contain the fissionable material in gaseous form. The temperature of the gas is high enough so that part of it is ionized. The direct conversion is accomplished by having the ionized (and therefore electrically conducting) fissionable gas do work against a magnetic field. The work done against the field appears as electrical energy in an external circuit. The dynamical motion of the plasma (hot gas) is so arranged that after each interaction with the magnetic field the plasma configuration is reshaped by solid walls so that magnetohydrodynamic instabilities cannot grow from cycle to cycle.

Basic Concept

Basically the reactor is a long cylinder of refractory material surrounded at each end by a neutron reflector and moderator. The cylinder is filled with a fissionable gas (say U^{233} or Pu^{239}). The amount of gas would be adjusted so that when it is uniformly distributed the system is just below criticality. Then any disturbance causing more

(Article taken from Nucleonics of August 1957)

CONCEPTUAL DESIGN of fission-plasma reactor. Cylindrical reaction chamber contains high temperature U^{235} gas at 1 atm pressure. Gas becomes critical when compressed at one end of cylinder. Resulting fission energy release creates shock wave driving gas to other end of cylinder where it is again compressed. Thus gas oscillates back and forth releasing fission energy at each end. As shocked gas travels along tube gas immediately behind shock front is highly ionized. Ionized gas, or plasma, through interaction with magnetic field transfers part of energy released in fission pulse to external electrical circuit. Energy appears as current induced in coil wrapped around cylinder. Energy is stored in LC resonant circuit formed by coil and capacitor bank until drawn off to external load. Carbon liner acts as thermal shield to keep D_2O temperature below boiling point. Reactor shown is minimum feasible size. Electrical power output would be 500 Mw

Direct Electric Power

fissionable material to be at one end of the cylinder than the other will result in multiplication and heating of the fissionable gas in the critical end. If the neutron-multiplication time is small compared with the time required for sound to travel the width of the critical region, then the temperature of the fissionable gas can increase by a number of times before a large fraction of the mass of the gas has been expelled from the critical end. Under these conditions a shock phenomenon will appear. The shock wave will travel from the heated or critical end down to the low-density opposite end. On the way it will pass through a magnetic field generated by a current-carrying coil surrounding the middle of the cylinder (see figure).

The figure shows the three regions that make up a typical shock wave. The shock disturbance, or shock front, is formed by very rapid heating of a volume of gas here called the "driving gas." The shock front travels away from its point of origin at some velocity greater than the speed of sound. The driving gas continues to expand and travels behind at a velocity somewhat slower than that of the shock front. There is then a disk-shaped region between the shock front and the driving gas that grows in length and travels at a velocity intermediate between the other two. This region contains the "shocked gas"—the gas that has been swept up by the shock since it started out. Characteristically the shocked gas is hotter than the driving gas. In the reactor this region of superheated gas will be sufficiently ionized to act like a conductor.

It is this disk-shaped conductor moving behind the shock that transfers energy to the magnetic field. When the disk of conducting gas enters the magnetic field it experiences a retarding pressure equal to $H^2/8\pi$ dynes/cm², where H is the magnetic field strength.* If the shock pressure is greater than the magnetic-field pressure, the conducting disk will be forced into the coil despite the magnetic field. When the disk moves past the center plane of the coil it feels a reverse pressure from the magnetic field that tends to expel it from the field.

During its approach, since the force on the disk is opposite to its motion, the disk does work on the magnetic field—some of its kinetic energy is transferred to the magnetic field. The magnetic field passes this energy on to a resonant condenser bank attached to the coil (see figure). As the energy is fed into the resonant circuit some of it can be drained off to do useful work, and some of it is returned to the disk through work done on the disk by the magnetic field as the disk leaves. The work done on the disk when leaving is always less than that done by it when entering. As a result the disk suffers a net loss in kinetic energy because of its trip through the coil. The energy lost by the disk appears (except for some unavoidable losses) as useful work in the external circuit.

The shock and gas flow continue beyond the coil into the opposite (so far noncritical) end of the cylinder. The flow will continue until there is considerable compression in this end. At some point in the compression sufficient fissionable material will be accumulated to become critical, the plasma will be rapidly heated and a reverse shock will take place. (more)

* In general a conductor moving in a magnetic field feels a force tending to push it in the direction of lower field. This happens because eddy currents are induced in the conductor that generate a magnetic field which cancels out the external magnetic field within the conductor volume. (Magnetic field lines cannot penetrate into an ideal conductor.) The interaction of these eddy currents with the original magnetic field behaves exactly as if the magnetic field exerted a pressure $H^2/8\pi$ dynes/cm².

The cycle therefore consists of a series of oscillating shock waves produced by the alternate criticality of either end of a cylindrical system. A magnetic coil oscillating with a condenser removes some of the kinetic energy of the gas flow behind the shock. The coupling between the magnetic field and the gas comes from the electrical conductivity of the extremely hot ionized gas.

Design-Parameter Estimates

To determine the engineering feasibility of such a device estimates are needed for the following design parameters: the operating temperatures of the gas and the container walls, the physical dimensions of the reactor cylinder, the density of the gas, the total power output, the strength of the magnetic field, the frequency of the critical oscillations, and the over-all power-plant efficiency. The physics of the situation is obviously quite complicated and involves these quantities in relationships that are not easy to describe mathematically. In fact a careful attempt to determine the physical variables of the system would be a very difficult task and would require supporting experimental investigation.

One can, however, start with a few reasonable assumptions and use "order-of-magnitude" arguments to obtain rough estimates of the interesting design parameters. This kind of approach produced the results shown in the table. The physical arguments used to obtain these numbers will be outlined later.

In deriving these estimates the following things were assumed:

- The gas is uranium metal (boiling point 3,900° K).
- The gas is surrounded by a cylindrical graphite container.
- The maximum heat flux through the graphite is 1 kw/cm², and the inside wall temperature is 3,500° K.
- The graphite is itself surrounded by a D_2O blanket that provides neutron moderation and reflection and removes heat from the graphite.
- The mechanical to electrical energy conversion is 70% efficient (does not include thermodynamic efficiency).

Although some of the numbers quoted in the table are extreme, none appears to be completely beyond reality. Certainly the construction of a fission plasma device seems feasible enough to deserve more thorough investigation.

Pros and Cons

Compared with a conventional reactor a plasma reactor would have the following advantages:

1. Since it is a low-pressure system and the supporting structure is at low temperature, the structural hazards are reduced.
2. The fissionable material is the working gas so that no heat exchangers need handle the radioactive material.
3. No prime turbine is needed although the same quantity of rotating electrical equipment will be used.
4. Because the fissionable material is at extremely low density, the prompt-fission multiplication rate is low enough to allow mechanical control of power excursions. In addition it can be shown that the criticality achieved by shock compression decreases once the radiation rate increases above the operating condition so that the reactivity has a negative temperature coefficient above the normal operating temperature.
5. Finally there is the distinct possibility that the thermodynamic efficiency can be raised up to 50%. This would be considerably higher than the efficiency of any conventional system.

The disadvantages are:

1. If U metal is used as the gas the wall temperature of the reactor must be very high, ~3,500° K. The use of carbon in this condition has not yet been sufficiently studied to determine feasibility, but other programs may develop the technology required. The discovery of a gaseous U compound that is stable at high temperatures would help this problem.
2. The minimum theoretical size is quite large. This makes a research effort difficult, but the size is not beyond that of modern large power plants.

The Basic Idea behind the Direct Production of Electricity

That electrical power is generated as a result of the interaction of the moving plasma with the coil-condenser oscillation can be understood in terms of the forced oscillation of a tuned electrical circuit. In this case the coil and condenser in parallel form a tuned resonant tank circuit. (See figure.) The oscillation is forced by the interaction of the induced eddy currents in the conducting plasma with the currents in the main coil. The eddy currents in the moving plasma are induced because the plasma is a conductor moving in a magnetic field. Since the induced current in the plasma is in such a direction as to give a net retarding force on the plasma, the electrical oscillations in the coil-condenser tank circuit are "forced" to a greater amplitude at the expense of the mechanical energy of the plasma motion. The consequent conversion of mechanical energy to electrical energy of resonant oscillation is the basis of electrical power generation.

Estimating the Parameters

The physical arguments used to derive the numbers shown in the table are intended to give order-of-magnitude design estimates rather than precise values.

Operating temperature. There is a good chance that UF_6 will prove to be stable enough at high temperatures to be used as the gas. However, we will derive the operating temperature assuming that the gas is uranium metal. The minimum ambient temperature will then be the boiling point of the metal or 3,900° K. The maximum temperature is determined by the rate of thermal radiation from the gas to the walls. An approximate estimate is that the gas will behave as a black body with an emissivity of 10% to 20%.* From an engineering standpoint the maximum heat flux that a carbon wall might take is 10^3 watts/cm². Assuming this much power is radiated to the wall, the black-body temperature for 10% emissivity is then determined by

$$\text{Radiated power} = 5.7 \times 10^{-12}\theta^4 \times 0.1 = 10^3 \text{ w/cm}^2$$

or

$$\theta_{max} = 6{,}500° \text{ K } (0.56 \text{ ev})$$

The carbon wall temperature can be about one half of this and yet maintain uranium-metal vapor at 1-atm pressure.

Plasma conductivity. The electrical conductivity of the heated gas is one of the factors that determine the mini-

* Even with a good neutron reflector the fission mean free path for neutrons in the gas will not be more than 20 times the width of the active region. If, as will be shown later, the fractional ionization is a few percent or greater, then the optical absorption cross section of the gas will be of the order of 10^3 times the thermal-fission cross section. The mean free path for photons will then be much shorter than the thickness of the gas so that the conditions for black-body radiation are satisfied. The fractional emissivity of 10% is due to the temperature gradient near the gas surface.

An alternative approach is to calculate the continuum radiation per unit volume of gas assuming the gas is transparent. One finds that this gives a much greater total energy flux than that obtained from the black-body calculation. Since the radiation rate can never be greater than the black-body limit, the black-body calculation is then justified.

mum feasible radius for the cylinder. If the fractional ionization is higher than 10^{-3}, the properties of the ionized gas (the plasma) rather than those of the neutral gas determine the conductivity (*1, 2*). The fractional ionization of the uranium gas at a temperature of 0.56 ev can be obtained by solving the Saha equilibrium equation for a plasma. If x is the fractional ionization

$$\log\left(\frac{x^2}{1-x^2}p\right) = -0.18 + \log\frac{g_2}{g_1} + \frac{5}{2}\log\theta - \frac{5{,}040V}{\theta}$$

where V = ionization potential of uranium in electron volts = 4.0 ev

$\frac{g_2}{g_1}$ = ratio of statistical weights of ionized state versus neutral state = ½

p = pressure in atmospheres

θ = temperature in ° K = 6,500° K

Putting in the numbers

$$\frac{x^2}{1-x^2}p = 3.3$$

A pressure of 1 atm (which is a likely pressure) gives a fractional ionization of 0.87. Since this is large compared with 10^{-3} the conductivity can be estimated by assuming the gas has the properties of a plasma.

Spitzer (*2*) gives the electrical conductivity, Ω, of a plasma in a magnetic-field-free region*

$$\Omega = \frac{\theta^{3/2}}{2\times 10^4}\ \text{ohm}^{-1}\,\text{cm}^{-1}$$

If θ = 6,500° K then Ω = 26 $\text{ohm}^{-1}\,\text{cm}^{-1}$.

The conductivity is related to the cylinder radius in that both influence the electrical efficiency of the direct-conversion process. Since the conductivity of the plasma is not infinite (not a perfect conductor), resistive losses will accompany the eddy currents set up in the plasma when it enters the magnetic field. These losses represent irreversible energy losses and therefore determine the efficiency of the conversion process.

The magnitude of the losses is related to the distance the magnetic field penetrates into the conductor. This distance is known as the "electrical skin depth." (For infinite conductivity the skin depth is zero; for finite conductivity the skin depth increases with decreasing conductivity.) The electrical efficiency is roughly the ratio of the area of the magnetic-field penetration to the total area of the plasma corrected for the fact that the magnetic field falls off as it penetrates into the plasma. For high electrical efficiency the skin depth must be small compared with the radius of the plasma.

Besides conductivity, the skin depth also depends on how fast the plasma travels through the magnetic field—i.e., how much time is available for the magnetic field to diffuse into the plasma. A rough estimate of this time would be the radius of the cylinder divided by the plasma velocity through the coil.

The expression for skin depth is

$$\delta = \frac{10^9 t^{1/2}}{8\pi\Omega} = 10^3(r/u)^{1/2}/8\pi\Omega = 1.2\times 10^3(r/u)^{1/2}\ \text{cm}$$

where u = plasma velocity in cm/sec

r = radius cylinder in cm

t = time in seconds

Order-of-Magnitude Design Figures for a Plasma Reactor

Cylinder dimensions	
Radius*	13 ft
Length	150 ft
Maximum operating temperatures	
Gas temperature	6,500° K
Graphite wall temperature	3,500° K
Gas density†	5×10^{17} atoms/cm³
Gas pressure	~1 atm
Total electrical power generated	4.8×10^5 kw
Magnetic-field strength	3,170 gauss
Velocity of expanding gas	1.6×10^5 cm/sec
Oscillation frequency	about 20 cycles/sec
Thermodynamic efficiency	30%
Over-all power-plant efficiency	20%

* Minimum theoretical value.

† Air density at STP is 5×10^{19} atoms/cm³

If we assume an electrical efficiency of 70% then the skin depth should be less than about 15% of the radius, or $\delta = 0.15r$. We have finally

$$r = 8\times 10^3\,(r/u)^{1/2}\ \text{cm}$$

or

$$r = \frac{6.4\times 10^7}{u}\ \text{cm}$$

Plasma velocity. The last equation shows that the dimension of the machine is smaller for a higher plasma velocity. One is tempted therefore to dilute the high molecular weight of uranium with an inert carrier gas of low molecular weight (e.g., helium) to increase the gas velocity. However, to have sufficient time for neutron multiplication, the shock speed must be less than thermal-neutron velocity (2.2×10^5 cm/sec). This restricts the addition of low-molecular-weight diluents.

If the gas density in the critical end of the reactor is at least twice the average density (as one would expect from the prior compression of the previous shock), then for a large, sudden change in temperature of the critical driving gas, the plasma speed should be approximately twice* the speed of sound in the cold gas.

The speed of sound in the driving gas is

$$C_D = 1.8\times 10^5\left(\frac{1}{M_{\text{eff}}}\frac{\theta}{278}\right)^{1/2}\ \text{cm/sec}$$

M_{eff} = effective molecular weight

θ = temperature in degrees K

Since the uranium is largely ionized the effective molecular weight is

$$M_{\text{eff}} = \tfrac{238}{2} = 118$$

and

$$C_D = 8.2\times 10^4\ \text{cm/sec}$$

The estimated velocity of the plasma is then

$$u = 2C_D = 1.64\times 10^5\ \text{cm/sec}$$

For 70% electrical efficiency the minimum radius of the cylinder turns out to be

$$r = \frac{6.4\times 10^7}{u} = 400\ \text{cm}$$

Gas density. The density of the

* When the shock, or disk of ionized gas, enters the magnetic-field coil, the induced current is normal to the magnetic field so that one would suppose that the electrical conductivity normal to a strong magnetic field would apply. This, however, is not the case because the electron scattering mean free path will be much shorter than the cyclotron radius in the magnetic field, so that the field-free conductivity applies.

* In fact the limiting velocity for infinite pressure ratio is $u = \frac{2C_D}{\gamma - 1}$ or $u = 3C_D$ since $\gamma = \frac{5}{3}$. This holds because the shock is driven by an adiabatic expansion of the driving gas.

fissionable gas in the cylinder is a crucial design parameter. The density must be adjusted so that the fission energy is released in a time short enough to form a shock wave at the critical end. This implies that the energy be generated within the time the fissionable gas takes to flow into and subsequently flow out of the critical end. The time for energy generation (t_g) will be of the order of twice the time for sound to travel the diameter of the cylinder, or $t_g \cong 2D/u = 10^{-2}$ sec.

There are two possible neutronic modes of operation that can give rapid heating of the fissionable gas. The most obvious is that the density of the U^{235} gas be high enough so that when it is compressed in one end of the cylinder about two neutron-multiplication generations take place within 10^{-2} sec (i.e., the reciprocal period $\alpha = 200$). To achieve such a fast period the peak reactivity must be 3 to 6 times critical. On the other hand, after the shock leaves, the neutron flux must decay by a factor of 10 before the shock returns. Otherwise the average neutron flux will be continuously increasing. However, the gas density falls by a factor of only 5 or 6 after the shock leaves. With such a high peak reactivity this is not enough decrease in density to give sufficient decay of the neutron flux within the shock-return time. The dual conditions of rapid heating and constant average neutron flux cannot be achieved by this mode of neutronic operation.

The second possible mode of neutronic operation is to arrange for the neutron flux to stay high and relatively constant in time within each reflecting end. In the region of the coil between the end, the neutron flux can be greatly reduced through leakage and absorption in the wall. When the bulk of the U^{235} gas is in either end, the high but constant neutron flux heats it, rapidly creating a shock. The subsequent expansion causes flow of the U^{235} gas out of this end but will not drag the neutrons with it. Since the neutron scattering cross section in U^{235} is a very small fraction of the total reaction cross section, the U^{235} gas leaves, but the neutrons stay behind. The low-density U^{235} gas left behind will of course see the same heat input per unit mass (the neutron flux is constant), but the temperature should fall rapidly because of the large change in radiation opacity. Consequently the return shock sees low-temperature low-density gas that can be again compressed.

It is not entirely clear that the constant-in-time neutron mode will support an oscillating shock; however, a slight oscillation of the neutron flux is all that is needed to ensure a shock behavior. Therefore a combination of the two neutronic modes is proposed. The oscillation of the neutron flux should be approximately 1.5 to 1 (i.e., $\alpha = 40$), which is low enough so that a steady power level can be achieved and also an attractively low-density U^{235} gas can be used.

The next step is to find what gas density will give an $\alpha = 40$ in the compression phase of the cycle.

First we will show that the neutron spectrum is thermal. The construction design envisioned (see figure) is that of a cylindrical tank 8 meters in diameter whose walls are lined with graphite surrounded by a thick blanket of D_2O moderator. The thickness of the graphite lining must be adjusted to maintain a temperature difference of 3,200° K for a maximum heat input of 1 kw/cm². The high-temperature thermal conductivity of graphite is ~0.1 cal/cm²/° K/sec. Therefore, the thickness of graphite becomes

$$0.1 \times 3{,}200/D = 1{,}000/4.18$$
$$D = 1.3 \text{ cm}$$

Since a thermal-neutron mean free path in graphite is 2 cm the neutrons that are returned by the D_2O blanket will not come into thermal equilibrium with the graphite. Instead they will enter the reacting system in an essentially room-temperature thermal (0.025-ev) distribution. (The high temperature of the uranium itself does not affect the fission cross section because the uranium velocity distribution is still slow relative to the thermal-neutron velocity.)

If time taken for both the slowing down of the fission neutrons and the reflection of the thermal neutrons is neglected, a simple estimate of the U^{235} gas density can be made. For a sphere 4 meters in radius surrounded by an infinite D_2O moderator, the return probability of a fission neutron is $P_r = 0.75$. The additional loss due to thermal-neutron absorption and loss in the middle region of the cylinder can be approximated by a factor $P_g = 0.9$.

Let ν = neutrons per fission of $U^{235} = 2.5$ and

$$\eta = \sigma_f/\sigma_a\nu = 2.5/1.18 = 2.1$$

To obtain an α of 40 a thermal neutron must travel an effective multiplication mean free path in a time $1/\alpha$ seconds. The multiplication mean free path is the absorption mean free path increased by the factor

$$\frac{1}{P_g(P_r\eta - 1)}$$

Therefore

$$\frac{v_n}{\alpha} = \frac{1}{\rho\sigma_a P_g(P_r\eta - 1)}$$

v_n = velocity of thermal neutron = 2.2×10^5 cm/sec

$\alpha = 40$

ρ = density of U^{235} in particles/cm³

$\sigma_a = 687$ barns $= 6.87 \times 10^{-22}$ cm²

Then the required density of fissionable gas is

$$\rho = 5.1 \times 10^{17} \text{ U}^{235}/\text{cm}^3$$

With this density the absorption mean free path is $1/\rho\sigma_a = 2.8 \times 10^3$ cm. Since the cylinder diameter is only 800 cm a thermal neutron will make on the average five traversals of the reacting volume before it is absorbed in the gas. The albedo of the reflector for thermal neutrons is approximately 98% (i.e., permitting 50 reflections before absorption in the moderator). The neutron economy is then good enough to afford considerable thermal absorption near the midregion of the coil, thus giving the required reduction in neutron density between the coils.

Cylinder length. The length of the cylinder must be great enough so that

Proposed Experimental Program

The design uncertainty most in need of experimental investigation is the effective electrical conductivity of the plasma. This has been estimated assuming an electron velocity distribution very close to thermal. High current density, large inelastic-collision cross section and large nuclear ionization are all effects that tend to cause electron velocity distribution different from thermal. (Fortunately these effects are in the direction of higher conductivity or smaller minimum size for the reactor.)

Unfortunately an experimental investigation must be made on equipment close to the actual size, but this is not so big a problem as might be imagined. A shock tube, 1 meter in diameter, 6 meters long, operating at 1 atm maximum pressure with a magnetic field of 5,000 gauss at the midpoint would give complete answers not only on electrical conductivity but on electrical cycle efficiency, radiation cooling and thermal conductivity as well.

the shock has a sufficient length in which to form and also long enough so that the neutron absorption at the coil region does not reduce the reactivity. A total length six times the diameter gives a length of three diameters at either end before the shock reaches the coil and also places the coil far enough from the ends so that the fractional neutron flux reaching the coil is small.

Power output. To obtain a crude estimate of the power output assume that one tenth of the time the shock is doing work against the magnetic field. Then

$$W = \text{pressure} \times \text{area} \times \text{velocity}$$
$$= 6 \times 10^5 \times (400)^2\pi \times 1.6 \times 10^5 \times \tfrac{1}{10}$$

or

$$W = 4.8 \times 10^5 \text{ kw}$$

This is an acceptable power rating but is rather large in view of the fact that the design is predicated on a minimum size.

Gas cooling rate. To maintain the oscillations the shocked gas must cool by roughly a factor of one half during the time the residual shock takes to travel from the coil to the sofar noncritical end. (The cooling will take place by radiation since thermal conductivity is negligible.) The magnetic field must reduce the shock velocity by roughly one-half so that the time for cooling is

$$t_c = 6 \times 400 \times 2/(1.6 \times 10^5) = 3 \times 10^{-2} \text{ sec}$$

The shocked gas disk will be about one radius thick so that the area and volume are

$$\text{Area} = 4\pi r^2 = 2 \times 10^6 \text{ cm}^2$$
$$\text{Vol} = \pi r^3 = 2 \times 10^8 \text{ cm}^3$$

The radiated energy at 10^3 w/cm^2 in 3×10^{-2} sec is

$$W = 2 \times 10^6 \times 10^3 \times 3 \times 10^{-2} = 6 \times 10^7 \text{ joules}$$

About one half of the work done by the driving pressure will appear as kinetic energy of the gas flow and one half as heat content. The heat content of the shocked gas is therefore 0.04 joule/cm^3, or 8×10^4 joules total. Since this is small compared with the amount that could be radiated, as calculated above, we conclude that the gas will cool rapidly enough to meet the requirements of the design.

It is evident that the radiation rate may not be optimum at this value. The cooling rate can be adjusted by:

1. Operating at a slightly different temperature.

2. Using a different radius and density.

3. Changing the length with other parameters fixed.

Magnetic-field strength. The magnetic field must be strong enough to reduce the shock speed by about one half. (Since the kinetic energy is proportional to the velocity squared, this removes 75% of the kinetic energy of fluid flow.) The value of the magnetic field must be such as to give a pressure roughly equal to the driving pressure of 0.4 atm, or

$$4 \times 10^5 = H^2/8\pi$$

or

$$H = 3{,}170 \text{ gauss}$$

External-circuit behavior. That the plasma actually transfers energy to the external resonant circuit can be shown by applying ordinary circuit theory. The coil in the figure is shown schematically in resonance with a capacitor bank. In practice, the capacitor bank would be a synchronous motor, but the principal of power generation is the same.

The oscillating shock wave can be thought of as a moving conductor that periodically changes the inductance of the coil. The capacitor voltage is

$$V = \frac{d}{dt}(LI)$$

where both L (inductance) and I (current) are periodic functions of the time. For a capacitance C

$$V = \frac{1}{C}\int I\,dt$$

or

$$\frac{dV}{dt} = \frac{I}{C}$$

Differentiating above and substituting

$$\frac{I}{C} = \frac{d^2}{dt^2}(LI)$$

If we let $x = LI$, then

$$\frac{d^2x}{dt^2} - \frac{x}{LC} = 0$$

Since the parameter L is a periodic function of time, the above equation is the Hill-Mathieu type, which, by Floquet's theorem, has the following solution:

$$x = a_1 e^{\mu t} p(t) + a_2 e^{-\mu t} p(-t)$$

where a_1 and a_2 are constant coefficients and $p(t)$ is a periodic function in time. The coefficient of the exponential is not determined, but the positive exponent term describes the case in which power is fed into the coil-condenser system. For this solution x becomes a periodic, exponentially increasing term. Since $x = LI$ and L is a bounded periodic function, then I is an unbounded periodic function. In practice the oscillations of I increase until the power dissipated into an external load becomes equal to the power input.

System efficiency. The electrical efficiency of the cycle is entirely determined by the resistive diffusion of the magnetic field into the plasma. The electric losses in the rest of the system should be negligible because large-size conductors can be used (i.e., the Q of the coil-condenser system can be made large). The diffusion loss in the plasma per cycle is the ratio of resistive loss to the mechanical work done. This is roughly the ratio of the area of flux penetration to total area of the plasma corrected for the fact that the magnetic field that diffuses at the outside radius is stronger than the average field. For this reason a 15%-of-radius field penetration will probably give a 70% efficient electrical cycle.

The thermodynamic efficiency can be approximated in the strong-shock limit by observing that one half the energy of expansion goes into thermal shock heating (irreversible) and one half into kinetic energy of fluid motion. If the magnetic field slows up the fluid flow by one half, it removes 75% of the kinetic energy, or 37% of the total energy of expansion. Radiation cooling will account for an additional 20% loss so that a probable thermodynamic efficiency of 30% seems likely. This results in over-all plant efficiency of 20%. The electrical efficiency can be improved by going to a larger size. The thermodynamic efficiency can be improved slightly by changing the effective γ of the gas with diluents.

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This work was supported by the U. S. Atomic Energy Commission. S. A. Colgate is primarily responsible for the electrodynamic cycle; L. Aamodt has calculated the neutronics of the gaseous reactor in much greater detail than appears here. William Ogle of Los Alamos contributed substantially to the early discussions.

The Prospect for Civilian Nuclear Powered Airplanes

By A. Kalitinsky

To place any discussion of the application of nuclear propulsion to civilian aviation is its rightful perspective, it is perhaps most appropriate to draw a parallel to the development of commercial jet transports. In the summer of 1939 engineers and scientists in Great Britain, Germany, and the U. S. were discussing the possibilities of jet propulsion, which had been proposed in England several years earlier by Whittle. Some laboratory work was in progress, but the first flight of an experimental jet airplane -- the German Heinkel -- was yet to come, and the first operational jet fighter -- the Me 262 -- was still five years in the future. Formidable obstacles to the development of jet propulsion, even for military purposes, were readily apparent. In fact, a high level committee of experts, convened in this country at that time to assess the potentialities of jet propulsion, recommended against the development of jet engines. Jet propulsion, the committee said, would only pay off at airplane speeds far beyond the capabilities of military aircraft known at that time. Even the most ardent enthusiast, if asked in 1939 to comment on the commercial prospects of jet propulsion, would have replied that they were dim indeed. The low take-off thrust of jet engines and the high wing loadings of jet airplanes would require runway lengths far in excess of those available at commercial airports. The tremendous fuel oonsumption and short life of the jet engines would make commercial operation unprofitable. The jet airplane would be, typically, a short-range aircraft, a jet fighter. Today, 21 years later, we have commercial jet transportation on domestic and transoceanic routes, and jet airliners promise to become more economical to operate than piston-powered airplanes.

Aircraft nuclear propulsion today is in much the same position that jet propulsion was in 1939. Much laboratory work has been done, but the first

flight on nuclear power, here or abroad, is still in the future. And many high level committees have been deliberating on whether or not to go ahead with the development of a nuclear powered airplane.

Anyone attempting to look into the future, past the immediate problems of achieving the first flight on nuclear power and developing its military applications finds himself, like his 1939 counterpart, in a difficult position. He can see the basic advantages of nuclear power, but he cannot, with any assurance predict the characteristics of the nuclear powerplants of 1981, their performance, reliability, and cost of operation. Not knowing these characterisitcs, he cannot be sure of the particular areas of usefulness in which nuclear propulsion will find civilian applications in the future, and can only delineate them in very general forms.

The main advantage of nuclear propulsion for aircraft derives directly from the great concentration of energy in a small weight of nuclear fuel. The range of a nuclear powered airplane is, therefore, not limited by the amount of fuel carried aboard, as is the case in a conventionally or "chemically" powered airplane. Once airborne, the nuclear airplane can fly without refueling for distances far in excess of those needed to link any two points on this planet. If extreme range is desired in a conventionally powered airplane, payload must be reduced and replaced by fuel, thus increasing the cost of transporting a given weight of cargo. The nuclear airplane, by contrast, can carry the same payload regardless of range.

A price must be paid, however, for the unlimited range and invariant payload advantages of nuclear propulsion. This price is the weight of shielding required to protect the airplane, its contents and environment from the lethal radiations originating in the reactors of the nuclear powerplant. This weight is quite large and, as a result, nuclear airplanes may be expected

to be large. On the other hand, the size of conventional airplanes grows rapidly as the payload and range are increased and the weight of chemical fuel needed to perform the mission is increased accordingly. Broadly speaking, then, nuclear powered aircraft may become competitive with conventional aircraft in the areas where the fuel load of the conventional airplanes is comparable to the shield weight of the nuclear airplanes. This is the area of very large aircraft, as large or larger than the biggest now flying.

Another consideration of importance in any civilian application is that of fuel cost. Although the nuclear powered airplane, as presently envisioned, could fly many times around the world without refueling, eventually the reactor cores must be replaced. This occurs long before all the uranium in the core has been burned up, due to the slowing down of the nuclear chain reaction by fission products and the deterioration of the core materials by heat and radiation. The used core must be processed chemically to salvage the unburned uranium and incorporate it into new replacement cores. The cost of this reprocessing may be even more important in determing the economics of aircraft nuclear propulsion than the initial acquisition cost of the nuclear powerplant. What this cost will turn out to be for future aircraft type reactors cannot be predicted at this time. All that can be said is that the fuel costs of nuclear aircraft powerplants must become generally comparable to the fuel costs of conventional jet powerplants before nuclear power can become truly competitive. There is good reason to expect this to happen. Reprocessing cost is presently one of the obstacles to achieving competitive electrical power from nuclear energy on the ground, and a substantial effort is being expended on improving reprocessing methods. This effort is bound to benefit aircraft nuclear propulsion even if indirectly.

Assuming for the moment that nuclear fuel costs can be reduced to where they are comparative to - although not necessarily lower or equal to -- conventional fuel costs; what are the possible areas of civilian applications for aircraft nuclear propulsion? As mentioned earlier, only those transportation tasks that would require very large conventional aircraft need be considered. This means the carrying of very large payloads over very long non-stop distances. Although it is not possible to put an exact lower limit on the size of payload, something of the order of 50-200 tons may be considered as representative. This may appear large in the light of present air cargo volumes. However, air cargo has proven to have many economic advantages, such as savings in time, inventory hold-up, and crating and handling costs, as illustrated by the recent air lift of small cars from France to the U. S. A market for air cargo capacity of this order undoubtedly exists if it can be provided on a competitive cost basis.

Turning now to the requirement for long non-stop distances brings us to the crux of the problem. Taking the trans-Pacific-U. S. -Japan route as an example, it could be flown non-stop by a nuclear powered airplane, or it could be flown by a chemically powered airplane with refueling stops at Hawaii, Midway, etc., where excellent airport and fuel supply facilities are already in existence. A direct comparison on an equal range basis is therefore not possible in this case. On the other hand, there are a number of air routes which have few or no facilities at the present time but which may become important with the economic growth of underdeveloped regions. Examples of such routes are South America to Southeast Asia or Australia, and Australia to South Africa. In some of these cases long non-stop ranges are required; in others the costs of establishing new refueling facilities would have to be balanced against the costs of non-stop operation, either chemical or nuclear.

Various studies have been made of possible nuclear powered airplanes to

meet requirements such as those sketched out above. A representative airplane might be one grossing about 500 tons and carrying a payload of about 200 tons. The economics of operating such an airplane can be compared with those of a chemically powered airplane designed to carry a payload of 200 tons for a distance of 2500 nautical miles (at longer ranges the payload of the chemical airplane is correspondingly reduced). In this comparison, a wide variation of possible nuclear powerplant and fuel costs must be assumed, since these are not adequately known at present. The operating costs of the nuclear airplane are independent of range, while those of the chemical airplane increase with increasing range. At some point, the cost of the chemical operation becomes equal to that of the nuclear operation; for greater ranges nuclear operation is more economical. Average assumptions lead to a break-even range of about 3500 n. miles. However, more pessimistic assumptions on nuclear fuel costs lead to break-even ranges of some 5000 miles, while more optimistic ones show as little as 2000 miles.

The average figure indicates that nuclear operation would be advantageous on the longer routes mentioned, such as South America-Southeast Asia, while chemical operation with refueling would be cheaper on the U. S. -Japan route. The optimistic figure could make nuclear operation attractive on a number of world routes, such as the North Pacific, North Atlantic, and U. S. to Australia, South America and Antarctica. The pessimistic figure, on the other hand, would make it unlikely that nuclear operation could become competitive except for military logistic transport or for special national purposes, such as supplying a large scale Antarctic installation.

The foregoing discussion has been concerned with cargo as opposed to passenger transport. Looking far into the future, when supersonic flight on nuclear power becomes possible, it is conceivable that a significant time

advantage could result for the supersonic nuclear airplane over the supersonic chemical airplane, since the latter may have to stop for refueling when operating over some of the longer routes mentioned earlier. Under these conditions, nuclear powered passenger transports may become competitive in some areas.

Before commercial nuclear powered cargo airplanes, and especially nuclear passenger transports, can be put into operation, several technical developments beyond those necessary for military applications must take place. A considerable saving in shield weight is realized by dividing the shield, i. e., by placing part of the shield around the reactors and part around the crew compartment. Most of the airplane outside the crew compartment is thus exposed to a significant level of radiation. This arrangement would not be acceptable for most commercial cargo items, and a heavier "unit" shield, placed entirely around the reactor will be required. This is technically not an insurmountable problem, but requires some further improvement in powerplant performance to compensate for the increased shield weight. An effective unit shield is, of course, a prerequisite for a passenger transport.

More difficult is the problem associated with the radiological safety of nuclear powered aircraft. In the event of a crash, the radioactive fission products accumulated in the reactor may be released and may endanger people at distances of several miles from the site of the accident. In military applications, the operating of nuclear powered aircraft can be restricted to areas of low population density where the radiological hazard is no greater than the present hazard to the civilian population from conventional aircraft operations. On the other hand, commercial operations to be economical will most probably require regular flights in and out of airports located close to highly populated industrial centers. To insure radiological safety under these conditions will

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require both considerable experience with military nuclear powered aircraft and new technical developments beyond the present state of the art.

In summary it can be said that the increasing interest in large volume air cargo and the accelerating economic development of many areas of the globe will create a future demand for large, long range cargo transports. Whether the nuclear powered airplane can meet this demand economically hinges above all on the operating costs of nuclear populsion, particularly the costs for building and reprocessing the reactor cores. Before nuclear powered cargo transports can become a reality, the radiological safety of nuclear airplane operations must be firmly established and assured. Finally, looking even further into the future, nuclear powered passenger transportation may develop from nuclear powered cargo airplanes.

(Thereupon at 12 o'clock noon, a recess was taken until 2 p.m., the same day.)

AFTERNOON SESSION

CHAIRMAN ANDERSON. The committee will be in order.

This afternoon is our fourth frontier subject, space propulsion and power. We have with us today some of the leading scientists and engineers in this field. I have expressed my views on the importance of accelerating our work in the development of nuclear rocket propulsion a number of times. I am pleased to say since our committee looked into this project last month some of the shortcomings of the Atomic Energy Commission's and the NASA's plan for the Rover nuclear rocket program have been corrected. Although additional funds have been made available for both test facilities and research and development work, we still have not set firm objectives and target dates for flight tests.

I am also not yet satisfied with the organizational setup for the direction of the overall project by an interagency group as adequate to meet the needs of this important project. I have reiterated my concern about these items to both the Atomic Energy Commission and the NASA by letters dated March 1, 1960. We intend to follow through to get these items taken care of.

Our first witness today is Mr. Krafft A. Ehricke, who is a pioneer in the rocket business. He started his work on rockets in Germany in the V–2 rocket program and at present is director of the program at Convair. It is a pleasure to welcome you at this time, Mr. Ehricke. May I just say in advance that one of the bills that is going to bother the Congress a little bit has just arrived at the Senate, and they are now sparring. I don't know whether they are going to take up civil rights today or next Tuesday. I may have to leave once or twice during the afternoon, and I hope that will be understood in advance.

Mr. Ehricke, I am very happy to have you here. I have told Mr. Ehricke in advance that I listened to the speech he made at the National Press Club a year or so ago, and since he agreed with me on so many things, I thought it was a masterful address. We naturally adjust these things to our own convictions. We really are very happy to have you here.

STATEMENT OF KRAFFT A. EHRICKE,[1] CONVAIR (ASTRONAUTICS) DIVISION, GENERAL DYNAMICS

MR. EHRICKE. Thank you, sir.

I would like to thank the members of the Joint Committee on Atomic Energy for the opportunity to discuss aspects of space propulsion system development, astronautic operations, and of their mutual interaction. As a matter of record I wish to state that I can-

[1] Director of the Centaur program for Convair (Astronautics) Division of General Dynamics Corp., helped develop the German V–2 ballistic missile during World War II.

Born in Berlin, in 1942 Ehricke received a degree in aeronautical engineering from the Technical University of Berlin. He became a research engineer for German Army Ordnance in June 1942 and worked on the V–2 rocket program at Peenemuende until May 1945.

Following World War II Ehricke was invited to participate in the U.S. Army's missile and rocket development program.

In November 1954 Ehricke joined Convair, starting as a preliminary design specialist on the Atlas intercontinental ballistic missile project. He was appointed director of the Centaur program in January 1959. Centaur is a high-performance upper-stage rocket, the free world's first space vehicle using liquid hydrogen as fuel.

not, of course, speak in an official capacity, since I am not an employee of any of the Government agencies involved; nor do I speak as a representative of Convair. My personal opinion is offered on the basis of many years of study of the problems involved here and with a deep interest in the soundness and quality of this country's long-range space program.

At first I would like to stress a number of basic principles which we can neglect only at our own risk.

1. The importance of quality over quantity in space propulsion work, as in every other aspect of space activity.

To illustrate this point, I call your attention to the frequent reference to Russia's superior thrust power as primary reason for our present inability to match Soviet space payload capability. It will, indeed, be years before the United States can match the thrust of Soviet space boosters. It will, however, be less long before the United States can match the Soviet payload capability. Atlas-Centaur which doubtless has less thrust than the large Soviet rockets will nevertheless exceed the Soviet payload capability displayed so far by a factor of about 3. The reason lies in the superior quality of the Centaur stage which operates on oxygen-hydrogen.

I think basically the same conclusion should apply on the long run to our propulsion philosophy in terms of nuclear propulsion and its superior quality in terms of energy concentration and payload capability over the chemical propulsion.

2. The importance of propulsion I would like to stress as a corollary to what I said before because I think our capabilities in the seventies and eighties in the field of long range space flight will be decided by what we are doing now, during this decade, in the field of nuclear propulsion development. The leap in quality level is essentially the switch from chemical to nuclear propulsion during this decade.

3. The importance of nuclear energy in mature space flight.

The universe is run by nuclear energy. Space will be conquered only by manned nuclear-powered vehicles. Planning anything else for the late sixties is, in my opinion, flirting with obsolescence almost from the start. At the risk of being repetitious, I would like to express again my strong conviction that the nuclear engine is for astronautics what the combustion engine is for aeronautics, the only means of obtaining complete independence operationally from the earth to the extent to which it is necessary in order to explore the planets of our solar system. I am firmly convinced that the surest way to cripple the U.S. space capability in the seventies would be to cripple its nuclear engine development in the sixties. It is therefore vital to give NASA and AEC the means to develop the nuclear propulsion system very vigorously.

4. The importance of our goals and our determination regarding space.

As far as the importance of our goals is concerned with which the propulsion program is closely associated, I would like to point out that the emphasis that I have just put on quality is not meant to negate the importance of quantity. In other words, the importance of large amounts of thrust. I would like specifically to emphasize that a project such as the Centaur is of extreme importance for us. However, a vehicle of this type should have been started in development around 1954–56, rather than 1959–60.

The importance of Saturn in my opinion calls for eventually raising the vehicle's thrust level and configuration from its present conception to the level of 3 to 4 million pounds of thrust using a new first stage structure and the Rocketdyne F–1 engines presently under development. These things, however, do not alter the fact that we are doing right now what we should have been doing or starting to do some 3 or 4 years back. Doing what we are doing right now might induce us to lose sight of the work which should be in progress now. Instead of having to work on a stretchout nuclear program, I believe AEC and NASA should be given the possibility of a considerably stepped-up effort in this field in the next few years.

5. The importance of large payload capability.

Another important point which ties closely in with nuclear propulsion is the importance of large payload capability. Once in space, especially in distant space, man is utterly dependent on his payload for survival, safety and for the fulfillment of his mission task as such. Specifically, large payload capability is important in the following areas. First, orbital maneuverability. That means here we have a large portion of the payload in terms of propellant for maneuverability. Second, protection against corpuscular radiation in space. Our shielding weights may be considerably higher than we anticipate at this time and this would go at the expense of our other payload, and would be critical if our payload would be basically limited. Third, mobility on the surface of other celestial bodies and also the capability of survival. If you land in a rocket vehicle with enough payload capability to carry a large nuclear-electric power source, then this system would be available to you for energy and life sustenance on the surface.

Fourth, emergency rescue missions into space. Once we are entering deep space flight with large crews, the possibilities of mishaps and failures far away from the earth exist, and we must make up our mind whether we let these people die out in space or whether we want to have a capability of rescuing them. It is perfectly feasible to organize on a rather short notice a rescue mission from the surface of the earth not only to the moon but also into interplanetary space. However, this will be possible only if we are using high thrust power and high load capability which again calls for nuclear propulsion.

Finally, I would like to point out the possibilities toward industrial and other utilization in space, which requires large payloads and the military potential which basically lies in the fact of having large payloads. Large satellite weights are less important for research satellites. Of course, the moment you want to have the option of using them or being perhaps forced to use them for military purposes, your degree of freedom is increased to the extent to which you have more payload available. In figure No. 1 (p. 263), I have outlined roughly the areas of payloads which will be required for the various types of missions. These payloads which I have shown in this figure are loads that should be available in a departure orbit for the particular mission in question, not necessarily the payload with which you arrive at your target.

This indicates the following. For earth satellites in research we can operate in the general region of 10 pounds to 2,000 pounds. Technical development satellites, technical laboratories in space, re-

quire weights in orbit of the order of between 2,000 and 20,000 pounds. For communication satellites at very great altitude such as the 24-hour orbit, we will need in a departure orbit near the earth payloads between roughly 6,000 and 20,000 pounds. For lunar landing probes and interplanetary probes the weight region is roughly the same, namely, 8,000 to 30,000 pounds initial orbital departure payload weights. This is for probes which are capable of transmitting pictures back from other planets.

If we become more ambitious and think in terms of manned lunar circumnavigation, then the initial weights we are talking about are of the order of about 70,000 pounds. For manned lunar landings with chemical vehicles, or perhaps even with nuclear vehicles, the initial payload is more of a minimum value of about 200,000 up to about 500,000 pounds. Interplanetary missions, of the type we might visualize as the initial phase, namely, fast interplanetary reconnaissance missions without landing on the target planet, lasting about 1 to 1.5 years, would require an initial assembly of a weight of the order of 1.5 to 2 million pounds in a near earth orbit for nuclear heat exchanger systems. These 2 million pounds would carry a net payload for the operation during the mission of approximately 150,000 pounds for a crew of about 8 people. This load which has to be assembled in orbit can be reduced to about one-half to one-fourth if nuclear electric propulsion is used, provided the electric spacecraft can be delivered into orbit in one piece. If orbital assembly is necessary, the resulting weight to be carried into orbit will be of the same order as that for nuclear heat exchanger drives quoted above.

For lunar base operations, and for Martian base operations and exploration of the outer solar system, we are talking of initial loads of between 2 million and 10 million pounds. These loads are quite obviously completely out of the reach of chemical systems.

You will notice in this graph (p. 263) that even vehicles as large as Nova barely reach the lunar landing capability level. For the interplanetary capabilities you find exclusively nuclear systems. At the lower level of interplanetary capability, the nuclear heat exchanger system and a combination between the heat exchanger system and nuclear electrical system such as the plasma rocket are applicable.

Finally, perhaps most important of all, the pulse system, fission or fusion pulse system, allows one to produce high thrust as well as high jet energy and thereby to carry out missions at a short time with high payloads. The importance of short mission periods is another point I would like to stress.

When we talk about missions to other planets in terms of 2 or 3 years, then perhaps we underestimate the stresses which will be imposed upon human beings as the result of the strict confinement in the very supreme loneliness of interplanetary space. I believe this is a problem which we have to cope with and one solution is to have more payload available, more space and more conveniences, rather than a mere incapsulation of man on a very small scale for years. We also may have underestimated the difficulties and hazards involved in keeping the spacecraft subsystems and man-life support equipment functioning over so long a time period, even if one takes a certain capability for repair into account.

Finally we may underestimate the hazards involved in exposing the crew to antibiotic space radiation environment against which complete protection by shielding apparently is not possible. For these and for a number of other secondary reasons, I am personally convinced that our capacity for manned operations in interplanetary space will grow or be frustrated in direct proportion to our success or the lack thereof in the development of propulsion systems which not only enable the emission of a high energy jet for high payload, but also produce enough thrust to reduce round trip times between the planets Venus and Mars to at least the order of 1 year, preferably less.

Again the answer is a nuclear propulsion system or a series or combination of nuclear propulsion systems.

In brief, I believe most of the points I made so far are interrelated. They add up to one principal conclusion, namely, that we would be wasting time and effort trying to prepare for interplanetary flight with chemical spacecraft. It is in principle possible to reach Venus and Mars with chemical vehicles. However, no true interplanetary nor for that matter even lunar base capability can be established in this manner. At least not to the degree as it could be ascertained if the same effort were concentrated on nuclear rockets. Our capability of carrying out manned missions to Venus and Mars in the seventies, if this is what we want to do, depends on the development of nuclear propulsion systems in this decade.

I have in my written statement outlined the physical fundamentals of a number of propulsion systems which we are dealing with. I would at this point like to group them into four classes. Firstly, systems which produce high thrust at low jet energy. These are obviously the solid and liquid chemical systems.

Secondly, systems which produce high thrust at medium jet energy. These are the nuclear heat exchanger systems in various forms. While the chemical system yields between 300 and 450 pounds of thrust for every pound of matter ejected, the second class provides between 800 and 1,000 pounds of thrust for every pound of matter ejected.

Thirdly, systems which produce high jet energy at low thrust. These are essentially the electric systems, plasma and ion. They may give you as much as 15,000 pounds of thrust per pound of material ejected. But you won't be able to eject more than a few millipounds. So you have high energy but very low thrust.

The final group, systems which produce high thrust and high jet energy, contains the pulse fission or fusion systems, the ones which use direct conversion of nuclear energy release into thrust. A direct application, I should say, without any detour into thermal or electrical systems through mechanisms such as turbines, generators and so forth, or heat exchangers.

Each of these systems has its place in the space propulsion spectrum. They are not necessarily mutually exclusive. In fact, they are quite complementary in the same sense in which a bicycle and a Cessna airplane and a jet liner are complementary. Together they add up to the desired flexibility, efficiency, and economy in space operations.

It might be useful to point out that all groups aside from the first one are based on nuclear energy. I would like to briefly characterize each one of these groups, if I may, in a few minutes.

There are a number of characteristic points about them. First of all in each group there is at least one technically feasible version in the sense that it can be developed into a flyable system by engineering and associated research efforts within the next 10 to 15 years or so, or perhaps even sooner. In the following I make reference to these systems rather than to those in each of these groups which are less clear yet from the engineering developmental point of view. The leadtimes for the development of these realizable versions vary but because of what I said just a moment ago, that they are technically feasible versions in each one of these groups, they are all of the same order of magnitude. To develop flyable propulsion systems and associated vehicle structures requires approximately, depending on priority and the degree to which subsystems, already developed, are used, the following periods of time.

Chemical systems around 3 to 5 years. Nuclear heat exchanger systems about 5 to 7 years. This is conditional. The condition is the use of reactors of the Kiwi type essentially of similar temperature levels and with thrust levels of the order of 70,000 to about 100,000 pounds. Anything bigger would require a somewhat longer leadtime. Electrical systems leadtimes depend on the system in question. For an ion system using an electrical power source which is already available it would take around 3 years to make it workable. If electrical power systems are not available and have to be developed together with the thrust device for the electrical propulsion system, then at least 5 to 7 years will be required. This excludes the flight testing.

The flight testing would have to be added to it. Large electrical systems of several pounds of thrust by the end of this decade or the beginning of the next decade will most likely best be tested in orbit directly. The test purpose would be combined with other measuring or operational purposes of this type of a vehicle. I believe that we will gradually lose the clear distinction between a test vehicle and operational vehicle in the future because the costs are very high, and one will therefore try to combine several purposes with the flight testing of a spacecraft.

The pulse systems are very difficult to estimate at this point. It is my personal and private opinion that if a very vigorous effort is made to develop a fission pulse system of not too ambitious performance characteristics that this could be done roughly in a decade.

The development cost is another important factor in the economy. Nucelar systems are basically, at least in their initial phases, far more expensive than the chemical systems. For instance, a pound of thrust by nuclear heat exchanger will cost you roughly 10 times as much as a pound of chemical thrust. So the advantage of the nuclear systems lies where you have very high energy requirements in your mission so that the chemical cost would go up by sheer weight of propellants, by sheer complexity of mission preparation. From the viewpoint of systems and mission economy—dollars per pound of payload—large nuclear-powered vehicles are more economical than large chemical vehicles.

The electrical systems have an advantage over the heat exchanger systems inasmuch as they can use small reactor types, although they are different types of reactors than the heat exchanger systems. Some-

thing of the order of a few kilowatts up to about 30 or 40 megawatts. Whereas in the nuclear heat exchanger system we are talking between 1,000 and 15,000 megawatts. It is several orders of magnitude greater.

In the heat exchanger system one has a relatively simple application of the nuclear energy merely by the rather crude method of exchanging the heat. Whereas in the electrical system one has a more complicated conversion mechanism. It is, as you well know, relatively easy to handle large amounts of energy in exploding a bomb—for instance, a hydrogen bomb—but it would be very much more difficult to handle even 1 percent of this energy in the form of an electrical output system. From the viewpoint of weight and complexity the electric system is inferior. From the viewpoint of using small versus large reactors, the electrical system has the advantage of greater elasticity. The nuclear system has the advantage of greater thrust and shorter mission periods in flying from Earth to the Moon, Venus, or Mars.

All propulsion systems except the electrical permit ascent from and descent to the surface of the Moon and of the inner planets. As for the planets which have an atmosphere, one would have to reckon with shielding problems encountered as soon as one enters the atmosphere with an operating nuclear system, because scattering begins and the amount of radiation shielding around the payload must be adequate. For lunar landings such restriction does not exist.

The nuclear heat exchanger system and the fission pulse system offer the possibility of a rapid interplanetary transfer through the inner solar system with large payloads. The electrical system and the pulse system offer the same possibilities for flights into the outer solar system. The pulse system has a greater universal applicability since it has fast mission capability and high payload for the inner as well as the outer solar system.

I have in table 2 (p. 262), which you find attached to the report, compared the principal domains of these propulsion systems, from the viewpoint of availability, development cost, Earth to orbit economy, Earth-Moon operations, short mission periods into the inner and outer solar system, establishment of bases on other planets, cargo transport into the inner solar system, and the capability of avoiding complicated orbital assembly operations for certain missions. You see, in conjunction with chemical vehicles we have frequently relied in our planning on the capability of having to assemble and actually build large spacecraft in an orbit around the Earth. This is a very difficult, complex, hazardous and uneconomical procedure. It obviously is the only way to do things if you have only chemical energy. If higher energy concentrations are available one has the possibility of avoiding these things. I have listed here, and this is perhaps for our long-range space capability the most decisive point, the areas in which one of these propulsion systems can avoid long and complex orbital operations in preparation of a deep space mission.

The electrical system is capable of delivering eventually up to 200,000 or 300,000 pounds into orbit with one flight. This gets into at least 4 million to 6 million pound vehicles with up to 9 million pound thrust.

A lunar circumnavigation without landing could be handled in this manner without orbital assembly. In other words, you ready everything on the ground, and you take off from the ground nonstop, without any orbital operations. You may hold over in a low energy orbit for celestial mechanical reasons, but this is another subject. The nuclear heat exchanger systems certainly can give you direct mission capability around the Moon and to the surface of the Moon and back. Into the inner solar system a direct capability without orbital assembly can be achieved either by very large nuclear heat exchanger systems or by a combination of nuclear heat exchanger systems as primary booster up to parabolic—minimum escape—velocity and a nuclear electric system which takes over subsequently to carry out the propulsion from there.

Finally, the pulse system gives you the capability of doing away with orbital assembly anywhere into the entire solar system for any type of mission.

Another point I would like to make is that electrical systems as well as other propulsion systems are mutually dependent and interrelated. As you develop one system you are paving the way for another system. As an example there is the development of the propulsion system for the Navaho vehicle, the first large thrust engines developed in this country, which then formed the basis, not for a family of air-breathing vehicle systems for which they were originally intended, but for our present IRBM's and ICBM's.

Another very famous example today is the development of the Jupiter and Redstone vehicle which now form the basis for the large Saturn booster.

Thirdly, the development of the Centaur vehicle, which uses oxygen-hydrogen, has paved the way not only for matching Russian payload capability, but has paved the way for a new design philosophy on upper stages which right now is being applied to Saturn and would also be applied to Nova. I believe it will contribute to the development of a nuclear orbital technology due to the use of liquid hydrogen in space. By the same token, I believe that the development of nuclear powered systems in the heat exchanger field as well as in the nuclear electric field will pave the way for the development of more efficient space vehicles as a whole. In other words, it will pave the way for efficient auxiliary power systems which can be used on the surface of the Moon or on other planets and, eventually, and most importantly, for the development of pulse propulsion systems.

I would like to conclude my prepared statement with this particular point. I believe that the gist of the basic philosophy which we should accept in the field of space propulsion and space vehicle developments during this decade should be one which has our desired capabilities and our desired potential in the seventies and eighties in mind. This potential, I believe, is an interplanetary and a planet surface exploratory potential. As such it can be based only on nuclear propulsion in one form or another. Therefore, I personally believe that one of the most important acts that we can do during this decade is to support vigorously the development of nuclear powerplants for propulsion purposes as well as for auxiliary power purposes.

Representative PRICE. Mr. Ehricke, in the early part of your statement you refer to the lack of interest and understanding regarding

our real important progress milestone against which the Centaur project was presented.

Mr. EHRICKE. Yes, sir.

Representative PRICE. Is the amount of interest, understanding and so forth, presently satisfactory to meet the basic principles which you list in your general statement as far as you are concerned?

Mr. EHRICKE. I believe that the present degree of interest in the development of nuclear propulsion is sufficient to sustain a larger effort on the side of NASA and AEC and the Department of Defense if it can be properly financed. I believe the primary difficulty here is to support this development properly by funding. I do hope that it is not necessary to sell most of the people concerned to the need for nuclear propulsion systems. There is always a threat, if you have a system which works, to go along this line and disregard as long as possible a new system which may involve more risks. Again this can be overcome only by encouraging the new development and especially the development of a demonstration system—in other words, a system which is capable of showing what is claimed that it is able to do. The development of a nuclear heat exchanger rocket system, no matter how comparatively simple or primitive it may be in this decade, will affect decisively the more advanced systems in the future.

Representative PRICE. Do you think we should establish an objective for achieving a flight test for a nuclear rocket?

Mr. EHRICKE. Yes, sir, very definitely.

Representative PRICE. Would you do this soon?

Mr. EHRICKE. Yes, sir. Soon I mean within our capability which I believe is something between 3 to 5 years for the first modest type of nuclear heat exchanger system. There is nothing as convincing as an accomplished fact.

Representative PRICE. It is important to set an objective for a flight test as quickly as possible; is that your opinion?

Mr. EHRICKE. Yes, sir.

Representative PRICE. In your statement you also said that you were on record with the technical publication back in 1954 showing the importance of a thrust level of 1.7 million pounds for the establishment of a first plateau of manned space capability, based on missile engines at that time under development. You emphasized that this was a point made by you in 1954. You left the implication there that although this point was thrown out by you in 1954, it was some time later before too much attention was given to it. What was the purpose of the technical paper that you prepared? Was it an official paper or just your own expression?

Mr. EHRICKE. At that time it was my own expression. There were developments underway at that time to use the engines developed for the Navaho for ICBM's. I made a systems analytical study on the thrust level that might be most suitable for a first plateau of orbital capability, including small manned stations. At that time I meant a crew of 10 to 15 people plus a lunar circumnavigation capability. I came to the conclusion that a thrust level of 1.5 million or 2 million pounds was a suitable one to start out with. Not very much higher and not lower. This is about the right level. I suggested that the development would be feasible at least in the planning stage at that

time with going into hardware as soon as these engines were fully developed and in use.

At that time this sounded like a rather tall order, since we were talking about considerably smaller amounts of thrust. At least in the past it did take approximately between 4 and 7 years if you threw out an idea, which was not just an idea, but based on some work and some studies, before that gradually could come to the point of realization. It took the statements of many other people in the same direction to bring it closer. However, in the ultimate analysis then I think it took the Soviet space accomplishments to really bring it up to a full-fledged project.

The point I wanted to make here is that I have so much emphasized the importance of propulsion systems over merely adding chemical thrust to larger and larger units, that this is not to mean that I am not very aware of our need for large thrust units as well. In my opinion the development of large chemical thrust units should have started a long while ago. We are doing it now. The fact that we are doing it now detracts from what we should be doing now, developing a nuclear system.

Representative PRICE. NASA has a 5 billion budget for Saturn and other rockets. The nuclear rocket project has less than $75 million scheduled for its budget and this is for a few years. Do you think this is a proper allocation of funds in the research of the two different types of rockets?

Mr. EHRICKE. Sir, I believe the nuclear side is too lean. I think it should be given more funding. There are two phases that we have to accomplish at this time. Phase No. 1 is to demonstrate a ground model of a nuclear heat exchanger system for use with liquid hydrogen. Phase No. 2, is that after this has been developed and has been demonstrated, the development as soon as possible of a first small nuclear-propelled rocket vehicle by 1964 or 1965.

Representative HOSMER. Mr. Ehricke, you have described roughly four different propulsion systems all dependent on thrust, tossed out the chemical systems, discussed the other three, and also acknowledged certain limitations on them. I am wondering if you have ever speculated about some kind of propulsion system for this purpose that does not depend upon thrust?

Mr. EHRICKE. I am sure I did not get the final portion of your question.

Representative HOSMER. I am wondering if you have ever daydreamed or heard anybody else daydream, science fiction stuff, if you will, about a space propulsion system that does not depend on thrust?

Mr. EHRICKE. Which is not producing thrust?

Representative HOSMER. Yes.

Mr. EHRICKE. Are you thinking here of a system of the type of the solar sail?

Representative HOSMER. No. I actually have in mind such a thing as might be described in the words of an antilight that would offer you an opportunity to obtain propulsion from light, or the antigravity sort of thing.

Mr. EHRICKE. Yes, sir.

Representative HOSMER. It is a rather odd question, but I have a reason to ask it.

Mr. EHRICKE. I think most of us in this field have speculated and thought about this at one time or another. I believe, however, that there is a general agreement among all of us that none of these systems to our knowledge at this time is even remotely ready for translation into technical practice.

Representative HOSMER. But there is some advance thinking along these lines?

Mr. EHRICKE. Yes. There is some advance thinking and it should definitely be going on. But it should not be tied to any particular project, space or otherwise, at this time. I think this is fundamental research about the nature of matter rather than a specific technical project.

Representative HOSMER. In other words, that sort of thing is in fact probably so far away that if we are going to get into outer space, we are going to be limited to the means that you described, as inadequate as they may be.

Mr. EHRICKE. Yes.

Representative HOSMER. Thank you.

Representative PRICE. Mr. Westland.

Representative WESTLAND. Mr. Chairman, it is good to see Mr. Ehricke again. I don't know whether you recall or not, but you and I made a television show about 2 years ago.

Mr. EHRICKE. Yes, sir.

Representative WESTLAND. Where you gave your theories on getting man into space. I might say that you created quite a furor in my district at least at that time with some of the prognostications you made. I recall one statement you made very well, when I asked you when you thought these things that you were demonstrating might happen, when we might get a man in space. As I recall it, you said that if you were to get the word it might be done in 2 years, and a space platform, as you demonstrated, perhaps you will recall, with pictures or artists' conceptions, in 5 years. We have had testimony from Mr. Glennan that they expect to get a man in space, perhaps technically, this calendar year. They expect to have a man in orbit next year. I wonder if you would comment on whether or not your time estimate is in tune, whether it is going along the way you anticipated or just where this picture is.

Mr. EHRICKE. Yes, sir. I believe it is very much in accord with the time estimate that I made at that time, especially for orbiting a man temporarily in a small capsule as a first step. I still believe that the first small manned space station being able to harbor approximately three to four people can be available in 5 years, in fact, less than that. In other words, I believe we have a good chance of making a small space laboratory project the direct follow-on to the Project Mercury at this time. There are many people at NASA who are thinking about such a project at this time. I think this development goes on very satisfactorily.

Representative WESTLAND. I am glad to hear that, Mr. Ehricke, and especially to have a prophet to come back a couple of years later and say that the things he was predicting at that time are really beginning to happen.

Representative PRICE. Mr. Ehricke, in your statement you say that it will indeed be years—you don't specify the number of years—before

the United States can match the Russian space boosters. What do you mean by years? How many years are you talking about?

Mr. EHRICKE. This depends a little bit on what we are doing. Let me give you an example. It is possible to operate present ICBM capability to the level of something of the order of half a million pounds of thrust. This is a technical possibility. I am not saying it is being done, but it is a technical possibility. Apparently half-a-million-pound thrust or thereabouts is what the Russians have been applying so far. Such a conversion would be made within about 2 years. The development of 1½ million pounds thrust booster, the Saturn booster, will enter the operational vehicle with a proper set of upper stages and this will take 3 to 4 years.

I would say that the time period within which we can match and/or exceed Russian thrust capability is about 3 or 4 years away.

Representative PRICE. How many?

Mr. EHRICKE. Three to four years away.

Representative PRICE. Three to four years?

Mr. EHRICKE. Yes.

Representative PRICE. Do you think there is any question about us being behind the Russians as far as the space thrust question is concerned?

Mr. EHRICKE. There is no question in my mind. We are definitely behind the Russians at this time. It is questionable whether we will be ahead of the Russians 3 years from now, because they don't stand still either.

Representative HOSMER. Mr. Chairman, I would like to ask again a comparative question. How do the two countries compare in relation to the information that they are getting out of space?

Mr. EHRICKE. I believe in terms of information we are even. I believe that we get at least as much information out of space as the Russians are getting.

Representative HOSMER. I think they have two vehicles orbiting at the present time, neither one of which is returning information, is that right?

Mr. EHRICKE. Yes.

Representative HOSMER. We have around six, I believe, of which four are returning information and will continue to do so for some time.

Mr. EHRICKE. Yes, sir.

Representative HOSMER. Does that indicate to your mind that we are actually getting more than they are?

Mr. EHRICKE. I think in terms of telemetered data we are comparable. We are making measurements about cosmic radiation, solar flares, micrometeorites, and so are the Russians. In this respect we have more space information coming out of space right now than the Russians. In terms of picture transmission, the Russians have one ahead of us. They have a picture of the back side of the moon, and we do not. I don't say that this is a world of importance at this time that we don't have it, and they do. In all of these scientific exploratory actions if the time element is not too much different, it is not extremely important. The importance is our capability in the long run in terms of very large payload. I believe at the moment our capability of doing space research, measurement of radiation, mag-

netic field, and other physical phenomenon is as good as the Russian capability. As soon as we enter the field of space exploration where more weight is required such as for picture transmission from the moon or the plants, we are at somewhat of a weight disadvantage at this time.

Representative HOSMER. To use the example of the picture, if you get back a picture from a thousand-pound space vehicle and the same picture from a 100-pound space vehicle, you have the same information. There is no difference in the quality of the information or the quantity of the information, isn't that right?

Mr. EHRICKE. That is right, if we can get a picture back from a 100-pound vehicle.

Representative HOSMER. Where are we in relation with the Russians in the question of miniaturization of technical electronic space data type of equipment?

Mr. EHRICKE. I believe we are ahead of the Russians in this respect. In other words, we get with less payload as much information as they get.

Representative HOSMER. Then that factor cuts down the differential of the mere pounds of available thrust.

Mr. EHRICKE. Yes. It cuts down the potential in this respect as far as our initial space exploratory capability is concerned.

Representative HOSMER. The long lasting time with which instrumentation can function and return information also has a bearing on cutting down any difference in thrust potential, does it not?

Mr. EHRICKE. Yes. You mean the duration of data transmission from a given probe.

Representative HOSMER. That is correct.

Mr. EHRICKE. Yes.

Representative PRICE. Mr. Ehricke, what is the importance, then, of us attempting to match the Russian thrust?

Mr. EHRICKE. As I pointed out at the beginning of my statement, I think what is important is that we match at least Soviet payload capability in space, not necessarily for measuring radiation densities, but for the larger missions to come. That is, manned operation in lunar and sublunar space. I should like to add that we should not only look at it in terms of matching or increasing the Russian thrust capability or any other capability, but in adhering to our own plans. I believe we are capable during this decade of extending man's reach up to the moon. In the next decade and the decade thereafter, to proceed out into interplanetary space. If we are not preparing and laying the groundwork, especially in the propulsion field, in this decade for doing so, we will not have that capability. This is primarily independent of whether the Russians will have that capability or not.

Representative HOSMER. Let us get this straight. You are not recommending any crash program on chemical fuel engines. What you are talking about in the way of necessity of developing an increased thrust is that to be gained from the nuclear operated propulsion system.

Mr. EHRICKE. I am recommending that we develop chemical engines to a thrust level as far as its usefulness for space operations reaches. In other words, chemical vehicles are good for earth to orbit and perhaps earth-around-moon type of missions. The thrust

levels which satisfy these missions requirements are up to approximately 3 to 4 million pounds of thrust. The next levels, namely, lunar landings, lunar base establishments and interplanetary operations with men involve mission energy levels which are in my opinion beyond the chemical capability and we should recognize that, if we prepare for these types of missions, we should switch from the chemical to the nuclear propulsion systems. We should do this as soon as possible in this decade in order to avoid crash programs which are always inefficient and expensive.

Representative HOSMER. Did you mention Saturn in your testimony?

Mr. EHRICKE. Yes. I think it is a very important vehicle which can potentially lead us right up to the chemical limit of the thrust capability of 3 to 4 million pounds.

Representative HOSMER. Let us assume something about the Saturn vehicle. Assume that it costs $50 million. Assume you have a 50 percent chance of getting out. That means, two launchings for one success, and the average cost would be $100 million. Do you think we could gain more by taking that $100 million and putting them on these nuclear engines, rather than trying to throw Saturn into space?

Mr. EHRICKE. No; I would not recommend that. I would definitely recommend that the Saturn program is not disturbed—in fact, that it retains the high priority it already has.

Representative HOSMER. In terms of the original part of your testimony, where you were comparing balloons with airplanes, you say that we are in this balloon stage and ought to push it to the extension.

Mr. EHRICKE. Yes; it is the extension. What I am trying to recommend is that before we definitely and seriously financially commit ourselves to any larger chemical projects than the Saturn, we should take a very serious look at nuclear propulsion and we should give the nuclear propulsion system a chance to demonstrate itself before we definitely and finally commit a large amount of money, in terms of billions of dollars, to a still larger chemical space vehicle.

Mr. RAMEY. Is it logical to assume that with the Russians having the lead in chemical fuels that they are not waiting around and going into nuclear rockets themselves?

Mr. EHRICKE. I do not know what the Russians are doing in this field, but I would be surprised if they would neglect this potential.

Mr. RAMEY. So at the time we achieved the Saturn possibly the Russians could be well along in the nuclear?

Mr. EHRICKE. They could be. I would say it is doubtful that the Russians will surprise us with a nuclear rocket by the time we have the Saturn. They may surprise us with a nuclear rocket by the time we would have a large chemical vehicle of the Nova type by the end of this decade. We have to look very carefully whether a chemical Nova or a Rover type vehicle is to be developed. This is really the question. Not the Saturn is the question, but what is coming afterward is the question and key issue.

Representative HOSMER. Let me ask just one more question. From your knowledge of what the Russian chemical systems are, is it better fuel combinations or more of it that gives the additional thrust?

Mr. EHRICKE. The Russians at this time, as far as I know, have their superiority based merely on quantitative superiority, not on qualita-

tive. In other words, they have bigger thrust engines. They have no better propellants or propulsion systems than we have. Our first action of accumulating a bigger thrust potential chemically is a very natural one, and a very good one. This is being done in the Saturn project right now. That is the best thing we can do. The question is, After we have done this in order to match their capability in this field, what next? Shall we go on adding chemical thrust to chemical thrust? Or shall we somewhere make the switch from the lower energy plateau to the higher energy plateau? In other words, make a qualitative step?

Representative HOSMER. Will you answer one more question, then? What is the easiest scientific job and what is the easiest engineering job? Going to higher thrust in the manner you have just mentioned, or going to greater sophistication in the payload instrumentation?

Mr. EHRICKE. Sir, I believe that these two questions are difficult to answer in one stroke.

Representative HOSMER. Isn't that actually what the difference between the United States and the Soviet approach to this thing has been?

Mr. EHRICKE. No, sir, it is not. In the long run you just cannot keep on miniaturizing all the time. It is impossible. In other words, if you want to establish a base on the moon or if you want to get people around the moon, you cannot miniaturize people. You cannot miniaturize their life support system. You can make it lighter. Eventually you have to face the need for hundreds of thousands of pounds of payload.

Representative HOSMER. That is very true. I am talking about unmanned vehicles to obtain information. To send men up you have to have the payload.

Mr. EHRICKE. We are holding our own against the Russians in terms of information from space at this time merely because I think that we are superior in terms of payload sophistication.

Representative HOSMER. That is the objective, to get information, today.

Mr. EHRICKE. Yes, sir.

Representative HOSMER. Irrespective of whether you get a bigger or smaller means to do it, these countries actually are on a par.

Mr. EHRICKE. Yes, definitely. I would say they are on a par as far as the measured information from space is concerned.

Representative HOSMER. From what we talked about before, we may be a little bit ahead because we are getting a little more information over a longer period of time.

Mr. EHRICKE. That is true at this time.

Representative PRICE. The greater the thrust, Mr. Ehricke, the greater the payload.

Mr. EHRICKE. Yes, sir, and the greater the mission distance.

Representative PRICE. This morning we had the possibility of placing atomic reactors on the Moon and on Mars. To do jobs like that in the years to come is going to require tremendous thrust.

Mr. EHRICKE. Yes, definitely.

Representative HOSMER. You could not get that with a chemical thrust.

Mr. EHRICKE. I doubt it very much. Theoretically you could. In practice it is such a Spirit of St. Louis type of affair that if the slightest thing goes wrong, you have no reserves. We have to get ourselves accustomed to operating in the seventies with hundreds of thousands or even millions of pounds of payload. It is the only realistic approach.

Representative BATES. You said a moment ago the greater the thrust the greater the payload capability. That is only if the quality is the same.

Mr. EHRICKE. If the quality is the same.

Representative BATES. You gave a reverse answer to that a little while ago when you indicated the Atlas-Centaur project.

Mr. EHRICKE. That is right. Both aspects have to be considered. For these very large payloads you need both higher quality of propulsion system and greater thrust.

Representative BATES. You indicated the timetable as far as the race on thrust is concerned. Would you give us your judgment on the race for payload capability? Where do we stand today and when do you expect that to close?

Mr. EHRICKE. Our space payload capability, I believe, will be about 2 years from now—roughly, between two and three times as large as the Soviet payload capability displayed up to this point. Whether it will be that 3 years from now, I don't know.

Representative BATES. When do you expect it to get about even? When these particular developments come into being?

Mr. EHRICKE. They will be even by the time that the Atlas and certain upper stage space vehicles have come about. This will be a 1- to 2-year time period.

Representative BATES. Two to 3 years from now you expect the payload capability in the United States to be three times larger than the present Russian payload capability?

Mr. EHRICKE. Yes.

Representative BATES. That is all.

Representative PRICE. The point is assuming the Russians stand still.

Mr. EHRICKE. That is right. That is the point I made.

Representative BATES. I said the present.

Representative PRICE. Thank you very much, Mr. Ehricke. You have been a fine witness and we appreciate your appearance before the committee.

(Mr. Ehricke's formal statements follow:)

Statement of Mr. Krafft A. Ehricke

of General Dynamics, Convair (Astronautics) Division, San Diego, California

I would like to thank the members of the Joint Committee on Atomic Energy for the opportunity to discuss aspects of space propulsion system development, astronautic operations and of their mutual interaction. As a matter of record I wish to state that I cannot, of course, speak in an official capacity, since I am not an employee of any of the government agencies concerned; nor do I speak as a representative of Convair. My personal opinion is offered on the basis of many years of study of the problems involved here and with a deep interest in the soundness and quality of this country's long range space program.

At first I would like to stress a number of basic principles which we can neglect only at our own risk.

1. The importance of quality over quantity in space propulsion work, as in every other aspect of space activity.

To illustrate this point, I call your attention to the frequent reference to Russia's superior thrust power as primary reason for our present inability to match Soviet space payload capability. It will indeed be years before the United States can match the thrust of Soviet space boosters. It will, however, be less long before the United States can match the Soviet payload capability. Atlas-Centaur which doubtlessly has less thrust than the large Soviet rockets will nevertheless exceed the Soviet payload capability displayed so far by a factor of about 3. The reason lies in the superior quality of the Centaur stage which operates on oxygen-hydrogen. Having been connected with this project from the beginning, I consider it a cause for concern to remember the objections, the lack of interest and

the lack of understanding regarding our really important progress milestones against which this project had to be defended.

2. The importance of propulsion.

It will be decisive for our space capability in the seventies and eighties of this century whether or not our propulsion development philosophy in the sixties will be guided by the primate of quality - above all foresightedness and faith in our capabilities.

3. The importance of nuclear energy in mature space flight.

The universe is run by nuclear energy. Space will be conquered only by manned nuclear powered vehicles. Planning anything else for the late sixties is, in my opinion, flirting with obsolescence almost from the start. At the risk of being repetitious, I would like to express again my strong conviction that the nuclear engine is for astronautics what the combustion engine is for aeronautics, namely, the basis for true superiority and utility in atmospheric flight. The chemical rocket engine is a carry-over into the space age from the missile phase, just like the balloon helped bring about the air age. Like the balloon, the chemical rocket will retain indefinitely its usefulness for special applications. However, true astronautics and nuclear power are inseparably connected. I am firmly convinced that the surest way to cripple the U.S. space capability of the seventies is to cripple its nuclear engine development now and in the critical years ahead. The ability to prevent this from happening is in your hands, gentlemen. As an engineer, I feel an important corollary is that we should not hesitate to proceed doing right away what eventually we will have to do anyway. Such decision will not only save time but money as well and provide us with what I am sure we will wish we had worked for 10-15 years from now.

4. The importance of our goals and our determination regarding power

My emphasis on quality, before, does not negate the importance of high thrust power. I am on record with a technical publication in 1954 showing specifically the importance of a thrust level of 1.7 million lb for the establishment of a first plateau of manned space capability, based on clustering uprated missile engines at that time under development. The point is that this was 1954. In 1959/1960 a project of this scale finally got under way. Saturn's importance for the U.S. space program in the sixties can hardly be overestimated. It should be allowed to grow beyond its present conception into a 3-4 million lb thrust vehicle, using Rocketdyne's 1 million lb thrust F-1 engine presently under development and a new first stage structure.

However, this does not alter the fact that we are doing now what should have been done at least 3 - 4 years ago. This might induce us to lose sight of the work which should be in progress now. Instead of having to work on a stretched-out nuclear rocket program, AEC and NASA should be given the possibility of a stepped up effort in this field.

5. The importance of large payload capability

Once in space, especially in distant space, man is utterly dependent on his payload for survival, safety and for the fulfilment of his mission task. Specifically, large payload capability, that is, useful load in any form, is important in the following areas:

(a) orbital maneuverability,

(b) protection against corpuscular radiation in space,

(c) mobility on the surface of other celestial bodies,

(d) emergency rescue missions into space,

(e) industrial and other utilization in space,

(f) provides a military potential, if necessary. It is not very important now, who has a heavier research satellite in orbit. However, in terms

of a military satellite, weight is of great importance.

Fig. 1 surveys payload weight requirements in a near-Earth orbit for various space missions. In other words, the weights given are those which, for the mission indicated, have to be initially available in orbit. On the left hand side are indicated the weights which can be carried into orbit by the individual vehicles named. Different vehicle groups are clearly indicated, the solid propellant Scout vehicle, the liquid propellant Atlas family of space vehicles, the liquid propellant Saturn family, the liquid propellant Nova and a nuclear heat exchanger system rocket and finally a family of fission pulse type vehicles. The group of chemical vehicles is seen to barely reach the level of manned lunar landing operations. The nuclear heat exchanger system by itself does cover up to about twice the payload capability of Nova at less initial weight.

6. The importance of short mission periods in interplanetary flight

When we talk in terms of two or three years round trip (mission) time for flights to Venus and Mars we may underestimate the stresses imposed upon human beings as a result of strict confinement in the supreme loneliness of interplanetary space where the Earth shrinks from a "world" to a star apparently as remote as any other of the myriads of stars facing the interplanetary traveler in every direction. I believe, that no loneliness on Earth can compare with this. We may also have underestimated the difficulties and hazards involved in keeping the vital spacecraft subsystems and his life support equipment functioning over so long a time period, even if one takes a certain capability for repair into account. Finally, we may have underestimated the hazards involved in exposing the crew to the antibiotic cosmic radiation environment against which a complete protection by shielding appears not possible. For these, and a number of secondary reasons, I am convinced that our capacity for manned operations in interplanetary space will grow -

or be frustrated - in direct proportion to our success - or lack thereof - in developing propulsion systems which not only enable the emission of a high energy jet (for high payload), but also produce enough thrust to reduce round trip times to the planets Venus and Mars to at least the order of one year, preferably shorter, and mission periods to the outer solar system planets to the order of 1 to 3 years.

Most of the points made above are interrelated. They add up to one principal conclusion, namely, that we would be wasting time and effort trying to prepare for interplanetary flight with chemical spacecraft. It certainly is, in principle, possible to reach Venus or Mars with chemical vehicles. However, no true interplanetary - nor, for that matter, lunar capability can be established in this manner; such as it could be ascertained if the same effort were concentrated on nuclear rockets.

Space vehicles can be propelled by a great variety of propulsion systems. They all amount, ultimately, to an energy conversion mechanism which has the purpose to convert energy from a form in which it can be stored in the spacecraft to energy of an exhaust jet. The energy can be made available by combustion, by exchange of heat between a nuclear reactor and a dischargeable working fluid, by electricity which either is used to produce intense heat in a gas thereby converting it into an electric conductor which then can be accelerated to high speed by electric and magnetic fields (plasma drive) or by electricity which is used to accelerate charged particles (in the simplest case, ions) to very high speed in an electrostatic field (ion drive). Finally, nuclear energy can be used directly for propulsive purposes, such as in the fission or fusion pulse drive. The principle of this system is to utilize the energy released by exploding nuclear charges immediately for driving the spacecraft.

The manifold propulsion systems can be grouped in four categories:

(A) Systems which produce high thrust at low jet energy:

Solid and liquid chemical systems.

(B) Systems which produce high thrust at medium jet energy.

Nuclear heat exchanger systems.

(C) Systems which produce high jet energy at low thrust

Electric (plasma and ion) systems

Controlled fusion systems

(D) Systems which produce high thrust and high jet energy

Pulsed fission or fusion systems

Each of these systems has its place in the space propulsion spectrum. They are complementary in the same sense in which a bicycle, a Cessna, a jet plane and an ICBM are complementary since they are being used for different purposes. Together they add up to the desired flexibility, efficiency and economy in space operations. It will be observed that all groups, aside from (A), are based on nuclear energy. I will attempt a brief discussion of these systems.

(1) In each group there is at least one technically feasible version in the sense that it can be developed into a flyable system by engineering and associated research efforts within the next 10-15 years or sooner. In the following, reference is made to these systems rather than to as yet less understood versions which also exist in each group.

(2) The lead times for the development of the realizeable versions vary, but, because of (1) are all of the same order of magnitude. To develop a flyable propulsion system and associated vehicle structure requires approximately (depending on the priority, the degree to which subsystems are used which have already been developed, etc):

Chemical systems: 3 to 5 years

Nuclear systems: about 5 to 7 years

Electrical systems:	about 3 years if an existing power generation system can be used; alternately, about 5 to 7 years
Pulsed systems:	about 8 - 12 years

(3) Development costs differ by nature of the materials and the degree of previous experience involved. Therefore, the chemical system is least expensive. Generally speaking, one pound of thrust from a nuclear heat exchanger engine costs about 10 times as much as 1 lb thrust from a chemical engine. Thrust from electrical systems will, in turn, be much more expensive than nuclear heat exchanger thrust because a comparatively large, complicated system is required for electrical drives to produce a minute amount of thrust. The development of a pulsed system is difficult to estimate at this time.

(4) Transportation costs, on the other hand, tend to move in the opposite direction from development costs, since they decrease always with increasing payload. A payload increase, however, is attained with increasing energy of the propulsion system. In this sense I would expect the cost per pound of payload in orbit or around another planet to be lowest for the pulsed system, second lowest for some electrical systems and most expensive for the chemical systems, provided nuclear heat exchanger systems are not used in small sizes, but from about 600,000 lb thrust upward.

(5) Electric systems are able to use very small reactors (fraction up to a few tens of megawatts, compared to thousand(s) of megawatts for the nuclear heat exchanger systems.

(6) All propulsion systems, except the electrical, permit ascent from and descent to the surface of the Moon, and of the inner planets.

(7) The nuclear heat exchanger system and the pulsed system offer the possibility of rapid interplanetary transfer in the inner solar system at large payloads. The electric system and the pulsed system offer the same possibility for flights into the outer solar system.

The four propulsion system groups are compared in Tab.1 against this background. Their principal "domains" are listed in Tab.2. This table is necessarily somewhat simplified, but reflects the basic trends as to the expected long-range applications.

The development of propulsion systems, even in different groups, is not entirely independent of each other. Classical examples are the USAF/North American Aviation Navaho project whose rocket booster engines form the basis of a good position of this country's long range missile program; and the Redstone/Jupiter development which now is the foundation of the powerful Saturn booster. Another example is Centaur which not only is our earliest hope for exceeding Soviet payload capability, but also lead to the family of Saturn high-energy upper stages and no doubt contributes to the development of nuclear powered spacecraft through its use of liquid hydrogen in space.

Centaur and Saturn, in turn, will form the instruments for the flight testing and the first applications of electrical and nuclear heat exchanger propulsion systems, together with parallel developments in the field of auxiliary power systems.

One attractive characteristic of electrical systems is that they can be usefully employed in very small sizes, something that would be prohibitively uneconomical for nuclear heat exchanger systems. This is possible with electrical systems, because they use very low thrust values. In order to give you a feeling for the accelerations and flight times involved, at least in the Earth's field, Fig. 2 shows two solid curves which correlate the time at which, respectively, the 24-hr orbit (19,300 nautical miles high) and parabolic escape velocity from the Earth has been attained with the thrust/weight ratio (acceleration). Take, for example, an electrical

space vehicle which weighs 10,000 lb and has a thrust of 1 lb. The acceleration is thus 1/10,000 or 10^{-4}g. Fig. 2 shows that at 10^{-4}g it takes a space vehicle approximately 55 days to climb, from a low altitude orbit at, say 100 n.mi., into the 24-hr orbit (a chemical vehicle would require 5.4 hours). Parabolic escape velocity would be attained after 90 days at a distance of 100 Earth radii (almost twice the Moon's distance). The Atlas-Centaur, for example, can deliver about 9,000 lb into a 200 n.mi. orbit. At an acceleration of 1/22,000 ($4.5 \cdot 10^{-5}$g) the required thrust would be only about 0.40 lb. Now, one can compute the electric power required for producing, for example, a stream of ions which leave the ship at a very high exhaust velocity, say, at about 160,000 ft/sec (specific impulse $\sim$5,000 lb (thrust) per lb (mass) ejected per second). This power turns out to be about 60 kw. Such power level is not prohibitive a few years from now and, in effect, is firmly under development by NASA and AEC. If one compares now the payload which the Centaur can carry into the 24-hr orbit as an all-chemical vehicle with the payload it can deliver if the chemical operation from the near-Earth orbit on, is replaced by an electrical spacecraft, one finds that not only is the payload increased, but this payload does not have to contain any electrical power supply. The electrical power generation equipment for the thrust device, which is not part of the payload weight, provides an ample amount of power for communication at some penalty in payload weight due to special transformer equipment required, since the volt/ampere ratio is different for the electrical thrust device than for the transmitter. In contrast, the smaller payload of the chemical vehicle has to encompass the entire power generation equipment as well. As a result one finds that it is advantageous, even for relative small vehicles like Centaur, to employ a final electrical stage for getting more payload and more transmitter power into the 24-hr orbit as well as to Venus and Mars. Dr. English

of NASA, who will appear before you later, will discuss such project in more detail.

I have intentionally refrained from discussing specific nuclear vehicle projects, although this would, to a degree, be possible in an unclassified hearing, in order to avoid detail which is more the effect rather than the cause of the philosophy outlined in this statement. The gist of it is that beyond a well developed growth version of Saturn, nuclear heat exchanger systems should very seriously be considered as principal propulsion systems for vehicles whose primary mission region is no longer terrestrial space, but lunar space and beyond. Consequently, AEC, NASA, and DOD should be given the means to carry out this most important development. Thank you.

Tab. 1 Comparison of Propulsion Systems in the Light of Particular Astronautic Requirements

Availability	Development Cost	Earth-Orbit Economy	Earth-Moon Operations	Short Mission periods Inner solar system	Short Mission periods Outer solar system
Chemical	Chemical	Chemical	N.H.E.[1]	Pulsed	Pulsed
N.H.E.[1]	N.H.E.[1]	N.H.E.[1]	Pulsed	N.H.E.[1]	Electrical
Electrical	Electrical	Pulsed	Electrical	Electrical	N.H.E.[1]
Pulsed	Pulsed	-	Chemical	Chemical	-

Establishment of bases on other planets	Cargo transport inner solar system	Complicated orbital assembly operations can be avoided and operation carried out directly from Earth's surface by system	for operations
Pulsed	Electrical	Chemical	Orbital
Electrical	Pulsed	N.H.E.[1]	Lunar
N.H.E.[1]	N.H.E.[1]	N.H.E.[1] + Electric	Inner solar system
Chemical	-	Pulsed	Entire solar system

[1] N.H.E. = nuclear heat exchanger (system).

Tab. 2 Principal "Domains" of Astronautic Propulsion System Groups

Chemical:	(a)	Instrumented probes to Moon and into the inner solar system.
	(b)	Earth-to-orbit transport
	(c)	Earth-to-Moon (smaller manned missions)
N.H.E.:	(a)	Instrumented probes into the outer solar system
	(b)	Earth-to-Moon transport and lunar base maintenance
	(c)	Venus and Mars, fast reconnaissance flights
	(d)	Fast emergency rescue missions to Moon and into the inner solar system
Electrical:	(a)	Long-term ($\gtrsim$ 2 years) satellite position control
	(b)	Interplanetary probes anywhere in the solar system
	(c)	Trans-Pluto probes
	(d)	Slow, large payload transport to Moon and into the inner solar system (with some hope, but no certainty as yet, of extending its capability to fast inner solar system missions).
	(e)	Fast missions into the outer solar system
Pulsed:	(a)	Very large loads from the Earth's surface into orbit, or very fast to the surface of Moon, of the terrestrial planets and of the moons of the outer planets.
	(b)	Interstellar instrumented probes.

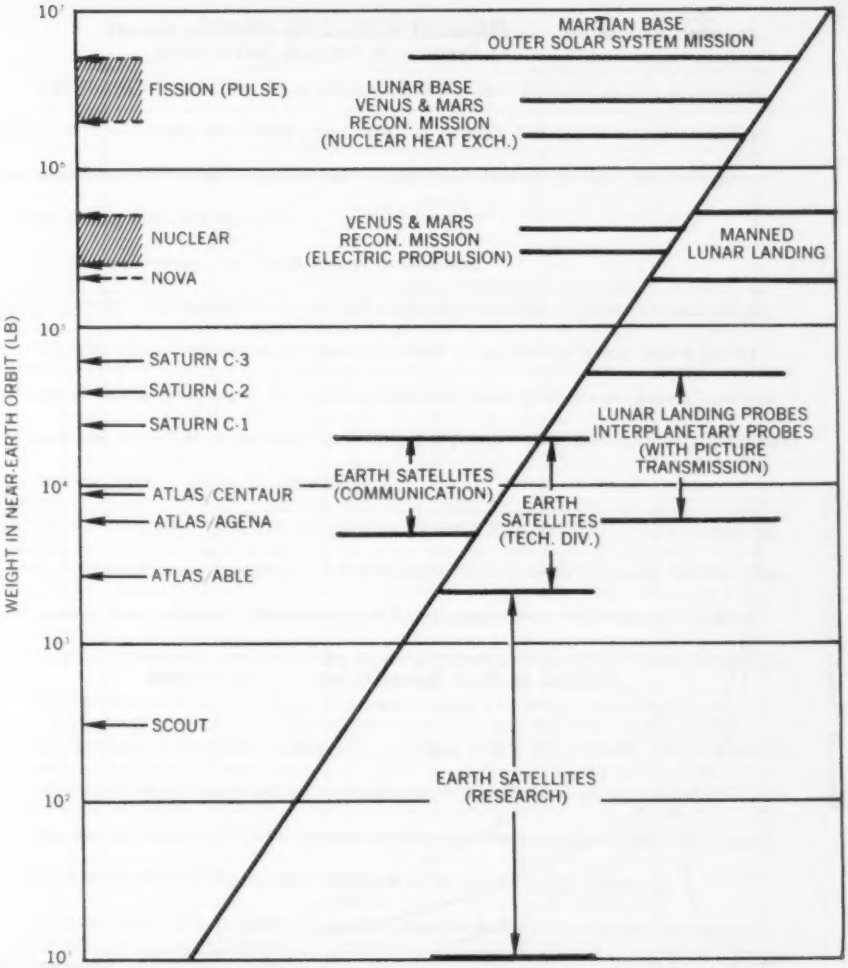
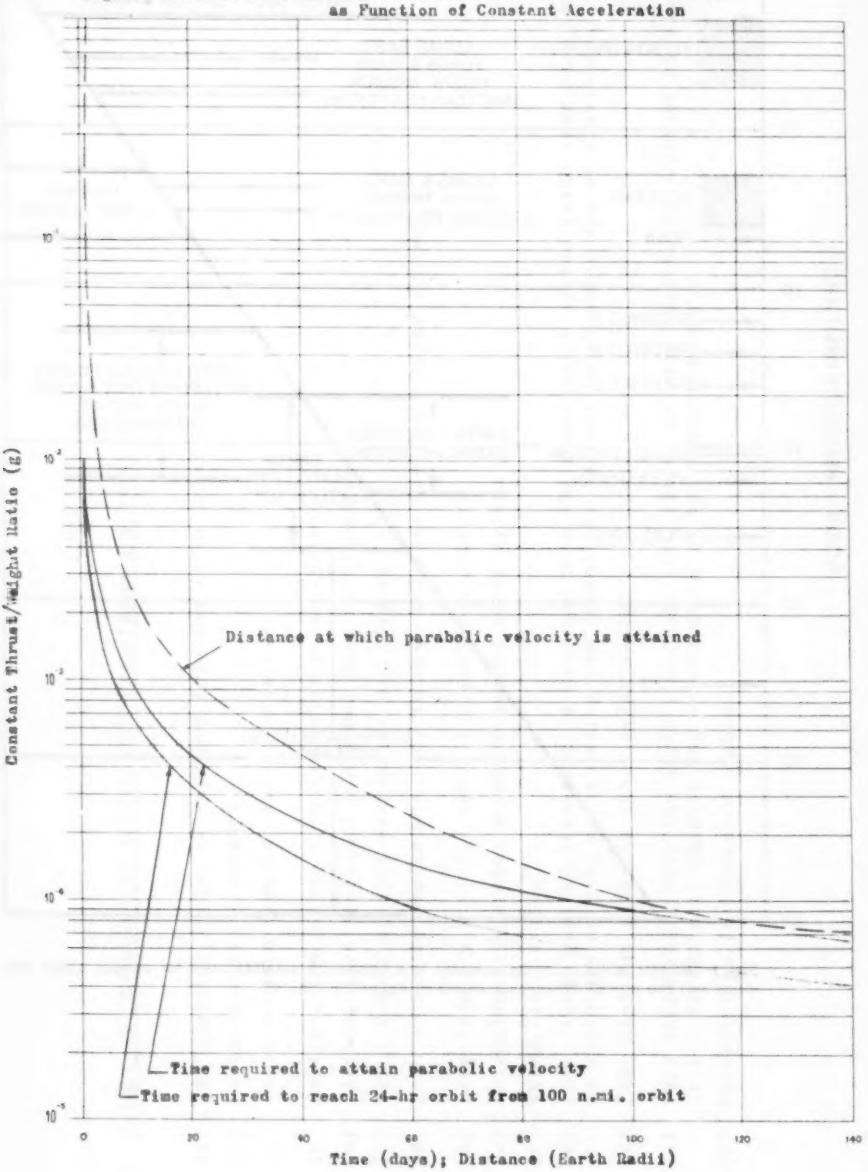


Fig. 1 Weights which must be available in a near-earth satellite orbit for various space missions and the orbital delivery capability of individual vehicles.

Fig. 12 Time of Powered Flight and Distance for Parabolic Cut-Off as Function of Constant Acceleration



Nuclear Power and Space Flight

Additional material on the significance of nuclear propulsion and secondary power is submitted to the Joint Committee on Atomic Energy in accordance with the Committee's invitation to file more complete statements than could be presented during the oral testimony.

In the oral testimony it was stated in essence:

(a) Since the development of large and advanced chemical rockets is well on its way (Centaur, Saturn) it is time to initiate a vigorous nuclear space power development program. The planned development of chemical space vehicles has been delayed unnecessarily during the fifties. We now have an opportunity to avoid this mistake in the sixties.

(b) The development of space vehicles leads their application to space missions by about 10 years. A significant portion of this time is required for the component development. The landing of lunar instrument carriers on the Moon and the circumnavigation of the Moon by piloted space ships in the latter half of the sixties is based on developments which are being carried out now and on the results of missile developments done in the late fifties. Our capability of establishing a base on the Moon or conducting interplanetary expeditions in the seventies and eighties depends on the development of nuclear propulsion systems and auxiliary power systems in the sixties and seventies.

Subsequently, the outline of a representative propulsion system development is presented, designed to provide a maximum spacegoing capability for the United States in the period 1970/90. Only high-thrust nuclear power will be considered here and its merits compared with those of low-thrust electrical systems for specific missions.

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Step 1: Ground Development

Surface demonstration of nuclear engine with liquid hydrogen, complete with hydrogen pump and pump drive. Based on the presently available reactor sizes, this engine could deliver a thrust of the order of 50, 000 to 75, 000 lb.

Step 2: Surface Launched Demonstration Model and Saturn Upper Stage

Incorporation of this engine system into a nuclear powered demonstration vehicle. It has frequently been proposed to make such vehicle the upper stage of a large chemical multi-stage system, such as Saturn. It alternately has been proposed to launch it from the ground. This writer believes that ground launching is preferable as an initial step, following, of course, exhaustive ground testing, (a) because of the many practical advantages this method offers in the development of a new device over air or space starts, (b) because the ground launching of comparatively small nuclear rockets is possible without costly radiation protective provisions on the launching site or in the payload section, since the demonstration model will not use a human or otherwise unduly radiation sensitive payload, (c) because this would allow accelerated development. It is believed that the feasibility of such a vehicle can be demonstrated by flight tests about 4 years from now. This means, that a large portion of this work could be carried on while Saturn is being developed and made sufficiently reliable to accept a nuclear upper stage. Hence, a delay of nuclear flight test work until Saturn is ready will be avoided. When both vehicles are ready, they should be mated into an operational chemo-nuclear system which offers a correspondingly increased performance with reliability and safety, even for a human crew.

Step 3: Large Nuclear Vehicle (Helios)

Development of an independent (i. e. not patched into an existing system) chemo-nuclear vehicle for a 300 n. mi. orbital capability of 250, 000 lb. payload. An optimized

vehicle proposed by this author is shown in Fig. 3. The nuclear vehicle is powered by a reactor of 12,000 to 14,000 megawatt, delivering a thrust of 750,000 lb. The engine is mounted underneath the hydrogen tank on the centerline. It is surrounded by a thrust structure which holds a ring of oxygen tanks and a number of chemical oxygen-hydrogen engines. These engines use hydrogen from the main tank as fuel. They provide the thrust required to lift the vehicle into the stratosphere, above 100,000 ft altitude, where the nuclear reactor can be turned on to full power without causing serious shielding problems to the payload due to radiation scattering, as it would be the case in the denser atmosphere. The purpose of the chemical booster engines is, therefore, merely to lift the nuclear vehicle to a stratospheric launching site, in order to avoid the costs, complexities and weight connected with a surface launching of such a large nuclear power plant. This author estimates the availability of a Helios vehicle in the flight test stage to be about 1970 if it is firmly planned. Fig. 3 shows the vehicle at nuclear take-off, jettisoning its chemical booster section. Tab. 3 compares the performance of this vehicle with that of a large chemical vehicle of the Nova class shown in Fig. 4.

The reason for proposing such a relatively large nuclear system as the next step beyond the demonstration model, rather than gradually replacing lower stages of chemical systems (e.g. Saturn second stage or Nova third stage) by nuclear stages is that nuclear stages are less efficient in terms of propulsion system/weight and performance advantage over chemical rockets when they are small. The only real justification for small size is demonstration. Thereafter a large-size propulsion system and vehicle should be developed to save time and money on less advantageous intermediate forms. Moreover, a vehicle of the Helios class does fulfill a number of important mission requirements which an intermediate form

FIGURE 3

FIGURE 4

FIGURE 3

would not, or not so efficiently, satisfy (Tab. 3). It would enable the delivery of 250, 000 lb into a near-Earth orbit; to land 80, 000 lb on the Moon's surface; to establish large automatic satellites around Venus and Mars in preparation of manned reconnaissance missions; and to carry out lunar and interplanetary emergency rescue missions of manned vehicles or installations in distress. Therefore, a large step is definitely more economical and faster and saves development time. In fact, this writer believes that the above described Helios vehicle is about the smallest size which can economically be justified for lunar supply missions. This will be shown below when comparing the economical aspects of the various vehicles.

Step 4: Interplanetary Booster (Urania)

Nuclear electrical propulsion systems are known to provide a high payload/gross weight ratio, but very low thrust, thereby causing the transfer times to Venus and Mars to be very long. A major portion of this time (about 4 months) is spent in spiraling out of the Earth's gravitational field. This can be avoided by delivering the electrical interplanetary into a parabolic orbit. This means that now parabolic (minimum escape) velocity rather than circular velocity becomes the proper reference velocity. A 10-man nuclear-electric spacecraft to Venus or Mars weighs approximately 500, 000 lb. To launch such a load into an escape orbit by means of a nuclear vehicle would require an initial weight of about 4. 5 million lb at an initial thrust of 6 million lb. The lift-off stage would again be oxygen - hydrogen. This stage would use 1, 440, 000 lb of liquid oxygen and 240, 000 lb of liquid hydrogen to lift the vehicle to an altitude of about 150, 000 ft where the nuclear engine cuts in and the chemical engines, their thrust structure and the oxygen tanks are jettisoned. Following the jettisoning of these parts, the initial weight of the nuclear vehicle is 2, 730, 000 lb at a nuclear thrust of 3, 500, 000 lb.

For parabolic (escape) payloads significantly in excess of one half million pounds, the required vehicle size and take-off weight becomes large enough to consider nuclear pulse systems of the Orion type.

Step 5: Fission Pulse Demonstration Vehicle (Orion I)

Step 6: Solar System Vehicle (Orion II)

The size and specific operation of these vehicles is presently classified. It has been stated, however, [1) 2)] that payloads of upwards to 1,000 tons can be delivered in orbit. At a specific impulse of several 1,000 lb thrust per lb/sec material expelled, payload fractions of 25 to 35 per cent should be possible with this type of vehicle. This makes the Orion far superior to any other nuclear or nuclear-electric system in terms of payload capability and shortness of flight time and far more economical in terms of dollars per pound of payload.

Roughly then, the Earth-launched vehicles can be divided, by their propulsion systems, into three groups:

Type	Escape Payload
Chemical	up to 30,000 - 50,000 lb
Nuclear Heat Exchanger	up to 300,000 - 500,000 lb
Nuclear Pulse Systems	1,000,000 to 10,000,000 lb

Nuclear electric vehicles must either be launched by nuclear heat exchanger vehicles of the Helios or Urania type, or they must be assembled piecemeal in orbit, forcing one to revert to what was considered in the past a necessary crutch

1) Secretary of Defense
2) Dr. Stanislaw Ulam, Oral testimony before the Joint Committee on Atomic Energy, Session on Space Propulsion and Power.

for deep space vehicles with chemical propulsion systems.

In conclusion, the economical aspects of vehicles with high-quality propulsion systems compared to vehicles with lower quality propulsion systems of very large thrust shall be compared briefly. Based on Atlas development figures an average cost figure of $150 per pound of hardware has been assumed. This cost is based on developmental vehicles of the type which recently was launched in a successful series of vehicles. For the nuclear reactor for the heat exchanger system a cost of $500 per megawatt has been assumed, corresponding approximately to $10 per pound of thrust. Propellant cost are negligible by comparison. The cost per pound of payload derived under these assumptions is listed in Tab. 4. The economic superiority of the nuclear vehicle is seen to become quite significant for the higher energy missions. A nuclear-electric vehicle is likely to be more expensive per pound of payload since its percentage of expensive hardware (notably electric conversion equipment and radiation cooling system) is significantly higher than for either the chemical or the nuclear heat exchanger vehicle. In the case of an orbital assembly of a nuclear-electric stage the cost figures are likely to be increased still further, because the cost of Earth-to-orbit cargo transport and of orbital assembly and check-out work (estimated to be at least $500 - 1,000 per labor hour depending on the length of time the person spends in orbit) must be added. The cost of nuclear pulse system is difficult to estimate at this time. However, it may be anticipated that the cost per pound of payload will be significantly less than for the nuclear heat exchanger vehicle, for lunar landing missions, and strikingly less for planetary landing missions, because of the very large payload weights involved. As Tab. 4 shows, the difference between the payload cost for the chemical and for the nuclear heat exchanger vehicle for the higher-energy missions is so large that it exceeds the tolerances which may be allowable due to the uncertainties in the above estimates.

Tab. 3 Comparison of the Helios with a Large Chemical Rocket of the Nova Class

Item	Nova-Class Vehicle	Helios
First stage	oxygen/kerosene	oxygen/hydrogen
Specific impulse $\left(\frac{\text{lb thrust}}{\text{lb/sec}}\right)$	286 (mean value)	380 (mean value)
Second stage	oxygen/hydrogen	hydrogen(nuclear)
Specific impulse $\left(\frac{\text{lb thrust}}{\text{lb/sec}}\right)$	420	830
Third stage	oxygen/hydrogen	-
Take-off weight	4, 000, 000 lb	1, 780, 000 lb
Take-off thrust	6, 000, 000 lb	2, 400, 000 lb
Second stage weight	1, 040, 000 lb	550, 000 lb
Second stage thrust	1, 200, 000 lb	750, 000 lb
Third stage weight	190, 000 lb	-
Third stage thrust	150, 000 lb	-
Payload capability		
300 n. mi. (polar)	190, 000 lb (2 stages only)	250, 000 lb
Parabolic escape	80, 000 lb (3 stages)	148. 000 lb
Soft landing on Moon	36, 000 lb (4 stages)	80, 000 lb

Tab. 4 Rough Comparison of the Cost per Pound of Payload for a Large Vehicle of the Nova Class and for the Helios Chemo-Nuclear Vehicle Described Above.

Item	Nova-Class	Helios
Hardware cost per vehicle ($)		
300 n. mi. orbit	38, 000, 000	30, 000, 000
parabolic escape	40, 000, 000	31, 000, 000
lunar landing	41, 000, 000	32, 000, 000
Payload cost (cf. Tab. 3 for payload weights) ($/lb)		
300 n. mi. orbit	193	120
parabolic escape	500	210
lunar landing	1, 140	400

Representative PRICE. The next witness will be Dr. R. E. Schreiber of Los Alamos Laboratory.

STATEMENT OF R. E. SCHREIBER,[1] LOS ALAMOS SCIENTIFIC LABORATORY

Dr. SCHREIBER. Thank you, Mr. Chairman. I appreciate this opportunity to say something about the Rover program. It is a little difficult to say enough in the short time available, particularly after listening to the last testimony, but I would like to present a very brief statement of what we are doing today and what our immediate plans are.

Nuclear rocket propulsion is a frontier of atomic energy research that is being explored by Project Rover. In at least some areas this frontier is fairly well explored and use of heat-exchanger nuclear engines to propel space vehicles seems to be a matter of detailed engineering, money, and a determination to carry through such a project. Many problems remain but these do not appear to be fundamental in nature. In order to make the most of a limited effort, the Rover program has concentrated on the simplest reactor concepts and the most available technology. As a result, there are many alternative approaches that deserve further attention, and these represent the relatively unexplored areas of the nuclear rocket propulsion frontier.

To digress for a moment on the sponsorship of this program, Project Rover was initiated as a joint AEC–USAF program, the AEC being responsible for the reactor development and the Air Force for nonnuclear engine and vehicle components as well as representing the potential customer requirements. In October of 1958 the Air Force role was formally transferred to the National Aeronautics and Space Administration, and this is the present arrangement. Since the beginning of the Rover work, the Joint Committee on Atomic Energy has given this program its vigorous support and encouragement.

I would like to depart from my text to state that the people in the development program have been keenly appreciative of this vote of confidence.

The simplest application of nuclear energy to rocket propulsion is the heat-exchanger reactor. It is essentially a gas-cooled reactor; that is, a reactor that has many coolant passages through which a gas can be pumped to remove the heat generated in the fuel elements. This hot gas is then discharged at high velocity through a nozzle at the rear of the reactor and provides the forward thrust for the vehicle. The thrust is proportional to the mass flow rate of the coolant gas or propellant and to its exhaust velocity or specific impulse. In actual application, the propellant is stored as a liquid and is vaporized by the reactor during the heating cycle.

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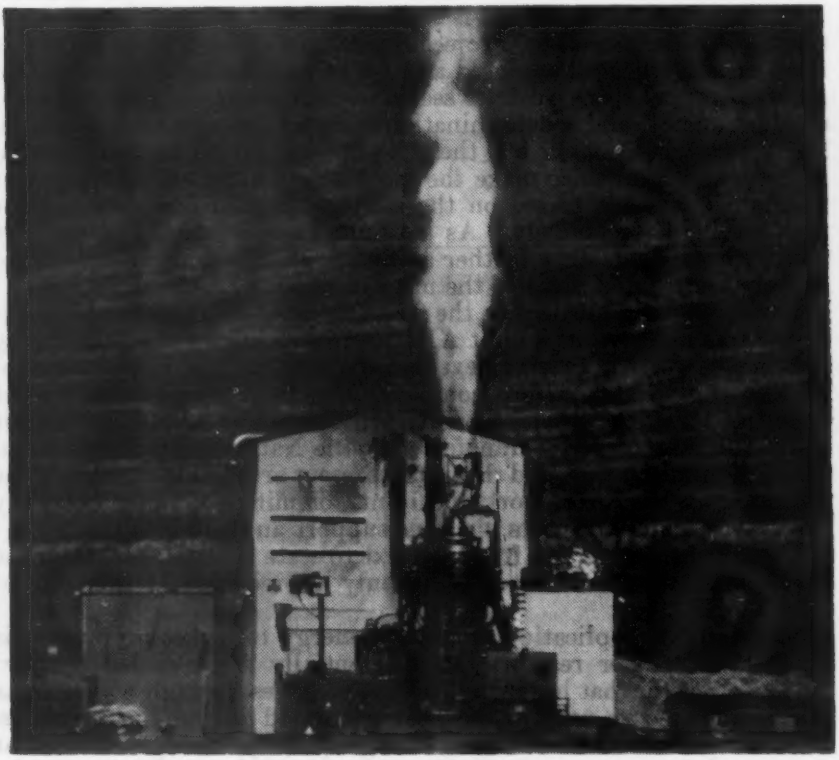
[1] Dr. Schreiber is a division leader in the Los Alamos Scientific Laboratory with a major responsibility for the design and development work in Project Rover. Prior to his present assignment he was head of the weapon engineering division at Los Alamos from 1952 to 1955. His first association with the atomic energy program was at Purdue in 1942 where he took part in an early investigation of deuterium-tritium fusion cross-sections. In 1943 he joined the Los Alamos Scientific Laboratory to work on the "water boiler" reactor and later was a member of the nuclear teams that carried out the field assembly and checkout of warheads for the Trinity test, Japanese combat shots, and Crossroads operations. Other assignments at Los Alamos have included supervision of critical reactor assembly experiments and of nuclear weapon engineering. He is a member of the USAF Scientific Advisory Board and the NASA Research Committee for Nuclear Energy Processes. He is a fellow of the American Physical Society and a fellow of the American Rocket Society.

Although simple in concept, a nuclear rocket engine operating at the temperatures and power densities required for useful space applications challenges the best technology available. A thrust of 100,000 pounds, for example, requires a power of about 2,000 megawatts, or 2 million kilowatts—considerably more than the electrical power requirements of the city of Washington.

Since this powerplant must be carried along in the vehicle, it must be as light as possible. In more technical terms, the power density of the engine must be very high. The specific impulse of the propellant, the pounds of thrust obtained per unit of mass flow rate, is a very important figure of merit and increases as the temperature in-



Kiwi–A in full-power operation on July 1, 1959. This picture is enlarged from a frame of a movie camera located approximately 500 yards from the test cell. The hydrogen exhaust flame extends considerably beyond the luminous central column. Since it is transparent, it can be observed in the photograph only by the blurring details of the mountains in the background. (Photo by Los Alamos Scientific Laboratory.)

creases. The propulsion reactor must, therefore, operate at the highest temperature possible. Consequently, one would like to have a nuclear engine roughly the size of an automobile that generates enormous amounts of power and operates at the temperature of a blast furnace.

The problem is eased somewhat by the fact that the operating lifetime is measured in minutes. However, it must be capable of being started and stopped quickly and precisely. Furthermore, many missions require that it go through several cycles of operation.

In view of these formidable requirements, can such an engine be built?

We believe that the engine can be built and can offer a successful test of the Kiwi–A reactor last year as evidence in support of this opinion. Although Kiwi–A had little resemblance to a flyable rocket engine, it required solution of the problems of control over a large temperature range and the development of fuel and structural materials to meet the strenuous environment of a high-temperature, gas-cooled reactor. The opportunity to compare design prediction and actual performance plus the actual experience of building and testing the complete reactor system was most valuable.

During the next few months we plan to carry out a fairly extensive series of experiments with a modified design of reactor known as Kiwi–A Prime in order to evaluate detailed design features evolving from the Kiwi–A experience. In order to determine the safety margins and limits of performance, we plan to continue testing until there is positive evidence of damage to the reactor. This type of failure can give very valuable experimental data. So I trust that the public will not be misled by distorted accounts of such events.

Work is also underway on a new generation of Kiwi reactors in which we start to put with the reactor the liquid hydrogen supply system, liquid hydrogen pump, regeneratively cooled nozzle, and simplified control system, all of these changes representing the transition from a reactor into a rocket engine.

In this connection, I should point out that the development of these nonnuclear components, in addition to specific items within the reactor, represent activities by private industry supported through AEC or NASA contracts under the technical direction of the Government laboratories.

A logical extension of the Rover program is the actual flight testing of a nuclear rocket propulsion system. Various possibilities include a vertical "lob" shot in which the total impulse would be deliberately limited to restrict the range of the test missile; and upper stage application in which the nuclear stage is placed in a trajectory or lifted into orbit by chemical boosters before it is started. The latter schemes require close integration with the national planning for large chemical booster development.

Another logical expansion of Rover work is the investigation of problems associated with very high power levels. The effects of drastic changes in size or power can be understood only by undertaking fairly detailed studies. Actual testing of large engines would require new facilities and a general expansion of the program.

The question that naturally occurs is this: Assuming that nuclear rocket propulsion can be accomplished, what does it do for us?

I wish to discuss this question in two parts that must be clearly distinguished. The first part relates to the ultimate potential of nuclear rocket propulsion, and the second to the gains that might be expected by applying the techniques currently available.

With regard to the ultimate potential, the important point is that the use of nuclear processes removes the energy limitation that exists in chemical systems. With the development of oxygen-hydrogen chemical engines, increases in performance are essentially at an end. With nuclear energy, on the other hand, the ability to provide a source

of energy in a small package is essentially unlimited. At the performance level of the oxygen-hydrogen system, for example, it would take more than 4 million pounds of hydrogen to use up the energy released by the fissioning of 1 pound of U^{235}. Increases in performance, therefore, depend upon finding ways to cram more energy into each pound of propellant.

The simple scheme of heating the propellant by passage through a solid-fueled reactor is limited by the temperatures of fuel elements. Liquid or gaseous-fueled reactors will extend the performance limits, but require the development of new techniques. The natural extension is into reactor-plasma combinations that might be developed by pooling the knowledge of the Sherwood and Rover programs. Such ideas are under study. If they could be worked out in a practical sense, they offer the opportunity to carry out ambitious space exploration far beyond present capabilities. How far we go in this direction depends largely upon the continued support of long-range programs exploring these difficult subjects.

Turning now to our more immediate capabilities, it seems generally possible to achieve about twice the specific impulse of the best chemical systems by means of nuclear propulsion technology now available. Using the oxygen-hydrogen chemical system of the Centaur type as a base, this means that nuclear stage should provide twice the incremental velocity of the chemical stage or that a nuclear stage should be capable of replacing two chemical stages or that the nuclear stage should perform the same mission as the chemical stage with a substantially smaller usage of propellant.

I use the word "should" advisedly in the above statement since it makes the assumption that the change from a chemical system to a nuclear system imposes no penalties in terms of additional deadweight. This assumption is not true because reactors tend to be much heavier than chemical combustion chambers and the low density of hydrogen and its cryogenic nature requires heavy tanks. Radiation shielding is required for some applications, and this also represents a weight penalty.

A detailed comparison of systems is a complex process and it is difficult to make broad statements that are not wrong in some respect, but perhaps a few generalities are worth attempting even though my colleagues in the missile development field probably will find them controversial. I wish to emphasize the fact that these generalities apply to present nuclear technology—not the ultimate nuclear potential.

1. Nuclear systems do not compete favorably with chemical systems for missions requiring only a few hundred pounds of payload placed in low earth orbits.

2. For low earth orbits requiring payloads of a few tons or more, nuclear systems are competitive with chemical systems and can accomplish such missions with a single stage rather than the two or three stages needed by chemical systems.

3. For more ambitious missions, such as moon landing and takeoff, interplanetary exploration, or establishment of stationary satellites, the substitution of a nuclear upper stage for one or two chemical stages almost invariably increases the payload by 60 to 100 percent. Replacement of the entire chemical system by a nuclear system leads to increases of payload relative to gross weight by factors of 5 to 10.

4. Very difficult missions such as rapid or extended space maneuvering, such as might be required to avoid high intensity radiation zones or to accomplish rapid interplanetary travel, probably can be accomplished only by means of some form of nuclear propulsion. The present levels of nuclear performance are barely adequate for such missions and the case for chemical propulsion is essentially hopeless.

In closing, I would like to emphasize the need for a balanced approach to programs such as Rover. We need to push ahead with simple systems to understand and solve the problems of translating design concepts to practical hardware. We also need to explore better techniques and new concepts in order to improve our usage of nuclear energy. The fact that we now know how to use only a fraction of a percent of the available energy is a challenge and a frontier that this country should not ignore.

I should also like to say that the high-temperature work, the reactor concepts, the material developments of a program such as the Rover project also turn out to be highly useful in other reactor fields and in other applications.

Thank you.

Senator ANDERSON. Dr. Schreiber, you know I am very much interested in this project. The Atomic Energy Commission got cut down some by the Budget Bureau, was able to get it restored if they could find funds, and I am very happy to say the Commission went out and found some funds they might use to keep this project going at the pace you planned. Are your scientists happy now again to know they can go ahead with it?

Dr. SCHREIBER. Yes, indeed.

Senator ANDERSON. Do you think that it had some beneficial effects out there?

Dr. SCHREIBER. Our worry now is translated into how we are going to spend this effectively, rather than where it is coming from.

Senator ANDERSON. You are not too worried about that?

Dr. SCHREIBER. We can find ways, I am sure.

Senator ANDERSON. I wrote a letter to Dr. Glennan and to others on the first of the month, in which I said that I was disappointed to find that objectives or target dates are still not established for the flight testing of a nuclear rocket.

As I indicated in my statement following the February 15 hearing, I believe it is extremely necessary to establish some firm objectives and target dates for the flight testing of nuclear rockets. In establishing this objective, these dates, it will be possible to work backward to develop our schedule for the next few years to achieve these goals. This will include bringing in an industrial contractor at an early date to work with Los Alamos Laboratory and certain reorganizational changes to direct this project.

Would you have any comment now on the possibility of setting some target dates?

Dr. SCHREIBER. I think the possibilities very good. In fact, things have been stirring rather violently since your letter was written.

In general, I would say that considerable progress has been made. This is my personal impression. I cannot be a spokesman officially for what is going on.

Certainly, the specific problems—the specific objectives are being looked at very carefully these days, and it is an active subject.

Senator ANDERSON. Specifically, if you were asked to set a target date for a ground test, it would not shock you too much, would it? You could fix some date even though you might not make it or might exceed it?

Dr. SCHREIBER. No. I would not be shocked. I would be very happy to try it.

Senator ANDERSON. We can work backward from the date if we get to it.

You mentioned the need for new facilities in the program expansion if we are to achieve the actual test date. Can you tell us anything about the status of those plans at this time?

Dr. SCHREIBER. My comment in the statement had to do with an expansion of this program to go to very high thrust levels. At the present time I would say that the facilities presently planned enable us to go through our ground test operations fairly adequately. The next stage in planning, which certainly must follow closely on an agreement as to our exact targets for flight testing, is to project that planning, both the technical and financial planning, to carry us through that phase. This is a part of the work which is now being looked at. However, I do not think that there is anything specific to report at the moment.

Senator ANDERSON. Doctor, I have made the statement a good many times—not being a scientist, nobody would pay any attention to it—that if we are going to take a ship off with people in it and land on the moon and then be able to take off again and bring it back, we probably would have to depend upon nuclear propulsion. I based that on the possibility that there is no atmosphere around the moon. Therefore, you are going to have to have retard rockets and brake the speed and land gently on the moon, and then be able to take off again.

When you see the pictures of the chemically propelled plants, you see stages dropping off, so that what finally goes is somewhat small.

I don't want you to commit yourself to any of my errors, but is there any feeling that if you are going to go to the moon and land a group of people there, let them explore a while and then take off and come back, that nuclear propulsion is somewhat essential?

Dr. SCHREIBER. I certainly think so. It seems to me this represents one of these intermediate types of missions in which it is certainly possible to stage enough large chemical systems to do that job. They become large and heavy; there are many stages that must work in series.

The large number of stages is definitely a disadvantage. It is certainly wrong to say that it could not be done—that is, done by chemical systems. I am naturally of the opinion that it would be far better to use nuclear systems where you do not have to have as heavy systems to start with. You have fewer stages where you perhaps have a bigger margin of safety because you don't have to strain everything to the utmost in order to accomplish the mission at all.

So it could be done both ways. I would certainly feel that the superior propulsion performance of nuclear systems offer the far better way of doing it.

Senator ANDERSON. I judge from what Mr. Ehricke had to say, that would be his feeling. The specific impulse is substantially higher, or we hope it will be, and thereby you will be able to carry additional directional devices and everything of that nature with it.

I am trying to ask you if you do not believe that this nuclear-propelled rocket offers very great advantages—not just a little scientific experiment—but that it could be very useful in a number of ways if this is speeded along.

Dr. SCHREIBER. You can look at any of the missions that require a large change in velocity—and by this I mean the order of 50,000, 75,000 feet per second—and in every case I think it is true that the high specific impulse available from a nuclear system offers you a large number of benefits in addition to simply the gross weight sort of argument. There are such features as I mentioned, of having too many stages in a low-impulse system, and the probability of losing a complete mission because of any single stage failure. If you have many stages, the overall probability of failure is greater.

There are many arguments of this general sort quite aside from the fact that the nuclear systems appear to be lighter and, in my opinion, in many respects simpler. These detailed considerations, I think, are something which we can only point to now. We cannot really prove any of these until we have gone a little further in our program. This is one reason I feel very emphatically that the sooner we can do a true flight test with a nuclear system, the sooner we will quit arguing about these things and be agreed on what we should do.

Senator ANDERSON. Thank you.

Mr. HOSMER. Dr. Schreiber, if you took one of these up to the moon with nuclear power and turned around and came back, taking off from the moon what would be the situation with respect to contamination?

Dr. SCHREIBER. This is a serious question. It is a complicated question. I will try to make a sort of simple answer to it.

If one, indeed, uses a one-stage device with very high specific impulse so it has a very large engine, so it has gone through a large power generation in getting there, then I think to get off and walk around such an engine is clearly impossible. You would have to have some way of shielding it or of separating the engine from the rest of the rocket in order to have people delivered in this fashion.

If you have a landing vehicle which has not gone through a large power generation, just enough to come to brake and sit down on the moon, then the nuclear radiation is something which can be tolerated with reasonable precautions.

Mr. HOSMER. I was thinking that there would be a large volume of hydrogen put through the reactor at the time of takeoff. That would be left as activated atmosphere on the moon. Would it likely dissipate off if, or what would be the situation for the next visiting firemen?

Dr. SCHREIBER. This presumably would disperse, so there would not be any hazard involved. It would upset the people who want to leave the moon completely pure in order that they may find out what it was like before people got there.

As far as a true hazard is concerned, I don't think what you are speaking of represents that.

Mr. HOSMER. Thank you.

Mr. PRICE. Thank you very much, Dr. Schreiber.

Dr. SCHREIBER. Thank you, Mr. Chairman.

Mr. PRICE. The next witness will be Dr. Stanislaw Ulam of the Los Alamos Scientific Laboratory.

56108—60——19

STATEMENT OF STANISLAW ULAM,[1] LOS ALAMOS SCIENTIFIC LABORATORY

Dr. ULAM. Thank you, Mr. Chairman.

Mr. PRICE. Will you proceed with your statement?

Dr. ULAM. I am very happy to be here again. A month ago there was a closed session of this committee, and I must say that Senator Anderson's committee established, if you want, this minor miracle—Los Alamos got more funds to work on Project Rover which Dr. Schreiber was just describing, and, before him, Dr. Ehricke. They were talking about various aspects of nuclear propulsion of rockets and space vehicles. Some of the discussion reminded me of debates that I am too young to have heard directly, myself, but I read about them. It was the controversy between lighter-than-air and heavier-than-air missiles—balloons versus airplanes. Now the situation is again somewhat similar.

We all agree that the chemical propulsion methods have accomplished great feats of space exploration, but undoubtedly the future of the more ambitious projects and missions belongs to nuclear means.

Project Rover belongs to one out of the four broad classes mentioned by Dr. Ehricke. It is being worked on at Los Alamos. It is a reactor. This reactor is—some of you might have had such impression—a possibility of the future. It is already here. All the data exist now to put up a nuclearly propelled vehicle right from the ground.

What I wanted to discuss today is a more ambitious project. It is known as Project Orion. May I make a few remarks about its history?

Shortly after the war, in Los Alamos, I was speculating about the possibility of using a pulse system. This would involve a vehicle from which you eject objects which explode—nuclear explosions. Bomblets, let us call them. Very small yield nuclear bombs. These would explode outside of the vehicle, and the gases of the explosion, or the materials surrounding these "bomblets" would strike a plate on the bottom of such a vehicle, and you do it very many times and each time you obtain say a 10-meter-a-second additional velocity increment. If you did it enough times, you would get to escape velocity and beyond.

It seems like a science-fiction scheme of the most wild and exotic type, but, if you look more closely at it, it becomes less fantastic.

The story is this: some years later Dr. Everett and myself at Los Alamos wrote a paper describing the properties of such a pulse system. Then Dr. Taylor, who, by the way, is here in the audience, took

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[1] Research adviser on the LASL Director's staff. He is a fellow of the American Academy of Arts and Sciences and an internationally known mathematician. Born in Poland, he received Ph. D. in mathematics from the Polytechnic Institute at Lwow in 1932. He is a member of the American Mathematical Society, American Physical Society, and past member of the Harvard Society of Fellows.

Ulam came to Los Alamos in 1944. Previously he had worked at the Institute for Advanced Study at Princeton, and lectured on mathematics at Harvard from 1936 to 1940. He has also been a member of faculty at Wisconsin, Southern California, Harvard, and MIT. He taught at various European universities before coming to the United States in 1936.

Ulam is widely known for his work on the development of the thermonuclear weapon and on the Monte Carlo method, a mathematical procedure for studying problems through probability chains. In addition to classified work, published works include articles on set theory, topology, functional analysis, ergodic theory, and theory of groups.

up this project at Los Alamos. Then, when he went to the west coast, he developed it further.

It appears that one can imagine now real ships with laboratories and observatories propelled in this fashion. These could be manned as it turns out in the scheme that is presently developed at the General Atomic Co. in La Jolla. The pushes would be comparatively mild, involving accelerations, say, of two or three G, which these people can stand. The first phase of propulsion would last only a few minutes. Then you could still have a lot of reserve power to maneuver, change the itinerary, and return.

One of the things discussed here before—a landing on the Moon, then taking off and returning—would be possible in this fashion. The payloads would be 1,000 or 2,000 or several thousand tons. So our explorers could travel in comparative comfort with abundant observational equipment and special laboratories installed in these ships.

It is not a case of scientific dreaming. Very thorough and conservative scientists, Dr. H. Bethe, for example, and others, looked at the technical details of this project, and it seems there is really nothing wrong with it.

Senator ANDERSON. Dr. Ulam, when you say a thousand, or several thousand tons of payload, what is the largest payload we have thus far put into orbit?

Dr. ULAM. We have put up some pounds.

Senator ANDERSON. Ninety pounds?

Dr. ULAM. Something like that.

Senator ANDERSON. You are talking about something that is 20,000 times that big?

Dr. ULAM. That is right. And it is not at all merely a fantasy.

Senator ANDERSON. At least that is an advanced project.

Dr. ULAM. It is. It is a real thing to aim at.

Every expert here today considered the present so-called missions and the information about space which we have obtained so far only as a preliminary to the real explorations of the future.

I would consider as premature now a discussion of uses or applications of space flights. It is like asking what was the mission of the Wright brothers when they took off on their first flight. The thing is the new applications which will come out of it.

Certainly, most of us here talk about true travel in space, the great advances of our knowledge of the universe, observations of the planets, and landings on the moon, putting observatories there. Another thing, some raise questions: Will there be some contamination of the atmosphere with radioactive products due to explosions of such bombs?

It turns out, from estimates made by Dr. Taylor's group, that it will be minimal in the atmosphere. Once out of the atmosphere, you need many more explosions, depending on where you want to go. But these will not contribute to the fallout.

According to some of my friends in this field—and I have this impression, too—the project is not a speculation about distant future, but it is something to consider for this decade.

People in General Atomic Co. at La Jolla have worked on it now for close to 2 years. A number of people, headed by Dr. Taylor, are very optimistic about the chances.

Coming back to Rover again, I would like to reiterate that, as far as I am concerned, nuclear propulsion is proved here. All that remains to do is to do it—and the sooner the better. It is apparently one of the biological urges of mankind to enlarge its knowledge and explore by travel as much of the universe as we can; and these are some of the methods which promise to enable us to do it very soon.

I would be glad to try to answer any questions.

Mr. PRICE. Doctor, you said in 1955 you and Dr. Everett wrote a report on this idea?

Dr. ULAM. Yes.

Mr. PRICE. And you stated it is still classified. Then you go on and tell about the plans and specifications and details for the construction of such a ship being underway at General Atomics.

Dr. ULAM. Not very much about how it is done or any details. Some of the general things about it.

Mr. PRICE. Are the details still classified?

Dr. ULAM. Yes, they are. The mechanisms and the various technological problems attendant to this method.

Perhaps I left you with the impression that all there is to do is to throw out a few bombs and let it go.

The whole scheme cannot be here described in a few lines, but the principle of the scheme has been mentioned publicly before. Some of it I repeated today.

Senator ANDERSON. Dr. Ulam, there was a publication put out this month by Douglas Aircraft, called "The Potential of Nuclear Space Transport Systems." In its conclusions—I want to read one paragraph and ask for your comment. The paragraph reads this way:

> The potential of nuclear space ships so far exceeds that of any other presently feasible means of space travel that development should be vigorously pursued at once. Any nation which achieves this capability first will achieve a space dominance which far overshadows any space achievements to date, for economical transportation is a far different operation than the current marginally justifiable programs. It is the real opening of a new frontier. Any nation which does not pursue it will be forever second rate.

That comes from Douglas Aircraft, a well-known industrial firm—March 1960.

Do you in general agree that nations should vigorously pursue this possibility of travel by nuclear space ships?

Dr. ULAM. Very much so. In fact, it seems to me that nuclear propulsion is the way in which we could show something really new. It is no fun merely to come up to where the Russians are now or where they were a year before.

I think there is here a bona fide question of national prestige. It is in scientific achievement. We should go ahead and show something really new this way which we can do.

That is the annoying thing about it: We can do it, and rather soon, given enough support. We can show something novel and useful and interesting. Why not do it?

Senator ANDERSON. Thank you, Doctor.

Mr. PRICE. Thank you very much, Dr. Ulam.

Dr. ULAM. Thank you, Mr. Chairman.

(Dr. Ulam's statement follows:)

STATEMENT BY DR. STANISLAW ULAM, LOS ALAMOS SCIENTIFIC LABORATORY

One of the most specific and striking applications of the development of nuclear energy is in its use for propulsion of rocket space vehicles. We are not on the threshold of the era of nuclearly propelled vehicles but already in its beginning. A month ago some of us, at a hearing of Senator Anderson's committee, described the successful experiments and tests which have been made for the so-called Rover program. Dr. Schreiber has talked (or will talk) about it. I would like to reiterate merely that one will have means to put many tons of payload into orbit around the earth and, very few years later, means to have lunar missions using exclusively nuclear propulsion.

Today I would like to talk about more ambitious projects. It seems certain that one can have much larger objects—manned space ships with observatories and laboratories—in interplanetary orbits in the near future, by using a somewhat different application of nuclear propulsion.

I am thinking of the so-called Project Orion. Shortly after the war, in Los Alamos, I considered a scheme whereby from a large vehicle small nuclear charges are ejected, exploded many times on the outside to obtain successive impulses on the bottom plate of a ship, giving it a necessary velocity for space travel. Some years later, in 1955, Dr. Everett and myself wrote a report (still classified) describing more details of such a scheme. Dr. Ted Taylor (who was in Los Alamos and is now, since 1957, in La Jolla at the General Atomic Co.) has developed and extended this scheme much further. It appears that a big ship, with payloads of the order of 1,000 or several thousands of tons, can be made to travel by such propulsion. He and Professor Dyson, from the Institute for Advanced Study in Princeton, who spent last year with this group of people and made many important contributions to this project, have shown many very interesting applications of such objects. I should add that in the scheme considered presently by this group, the accelerations given to the vehicle will be comparatively mild—of the order of 2 to 3 G or so. These would last altogether only a few minutes, so that the crew of such a ship would be easily able to stand it. Such ships could maneuver on all kinds of orbits, obtaining changes of direction by additional impulses of which there could be a large reserve. The possibility of having telescopes and other instruments on this large ship would promise immense progress in our knowledge of the universe. One could have landings on the Moon or satellites of other planets. Great extension of our knowledge of facts about the stellar universe would result from such observations.

These things are not merely a projected vision of theoretical possibilities. Detailed plans for construction of such a ship and some experiments on such means of propulsion have already been made by this group of people in General Atomic. In Los Alamos some experiments relevant to nuclear propulsion in this fashion have been started. The project to construct the ship would be a large one. I think the studies of it should continue and so should some mockup experiments using ordinary chemical explosions to imitate the action of repeated impulses to study the ablation of the exposed surface, etc.

One word about the possible dangers of contamination and exposure to radiation from the explosions for the crew of the vehicle. The result of calculations is that the total radioactive fallout in the atmosphere would be negligible. The exposure of the crew would be also far below limits of tolerance because of shielding by intervening material. These planned vehicles are very large affairs. It is not a question of "space capsules" but comfortable quarters for occupants of such a ship. Presumably the first attempts will involve unmanned vehicles propelled in this fashion.

Mr. PRICE. The next witness is Dr. Robert H. Fox. Will you proceed?

STATEMENT OF ROBERT H. FOX,[1] LAWRENCE RADIATION LABORATORY, UNIVERSITY OF CALIFORNIA, LIVERMORE, CALIF.

Dr. Fox. Mr. Chairman and members of the committee, it is a pleasure to be here to discuss the problems related to electric propulsion. We have heard discussions of the use of high temperatures for producing high exhaust velocities in rockets. Another idea which is actually about 30 years old is that of first producing electric power from some energy source and then using the electricity to accelerate the propulsion fluid to very high velocities. We can show that in this way extremely high exhaust velocities can be achieved, much higher than are required. In this case, if the exhaust velocity is much larger than the change in velocity required for the space mission, we find that nearly all of the vehicle can be payload. This is the principal advantage of electric propulsion and the reason for interest in it at the present time. There are, however, some practical limitations. We find that an electric powerplant in space turns out to be very heavy; that is, it does not produce much electric power for each pound of weight. We also find that high exhaust velocity means reduced thrust for a given power output, and that therefore the acceleration is very low. In fact, the kind of accelerations we come up with are about one ten-thousandth that of chemical rockets.

Now there are some important consequences of this low acceleration. In the first place, the device has to be boosted into orbit by some other rocket because it cannot lift itself. Mainly for this reason there has not been much interest in electric propulsion until the advent of satellites, but, of course, today we find many laboratories doing research work in this area.

In the second place, even after the device has been boosted into the low altitude orbit, say 200 or 300 miles above the surface of the earth, because of the low acceleration many days are required for the vehicle to escape completely from the earth. What the vehicle does with this low acceleration is to gradually increase the height of its orbit. It goes out in a slowly increasing spiral until after many days or weeks and several hundred revolutions about the earth it breaks free completely from the earth's gravitational field. This is shown here on this chart (figure 1) for an orbit over the poles. The light area here is the Van Allen radiation belt.

This low acceleration and the time required to go out would not be a great problem if it were not for these belts which contain many high-energy nuclear particles called protons. There are so many of these that a person could survive only a few hours in this belt without shielding for stopping them. In fact, protecting a person for many days requires several tons of shielding, so that this electric propulsion system does not appear very useful for transporting humans through the Van Allen belt.

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[1] Dr. Robert Fox received his doctor of philosophy degree in physics from the University of California in 1950. He remained at the University of California Radiation Laboratory for a year to participate in Operation Greenhouse and then went to Lincoln Laboratory, MIT, in September 1951. While there for 3 years he directed the development work on advanced microwave components using ferrite materials.

Returning to the University of California Radiation Laboratory in November 1954, he immediately began working on the general problem of the application of nuclear energy to propulsion in space and has spent part of his time on this problem up to the present. He has been a senior member of the Nuclear Propulsion Division of Lawrence Radiation Laboratory since the Division was organized in 1955 and is now assistant division leader for reactor research and advanced concepts.

During 1958–59, Dr. Fox directed a study of electric propulsion for the AEC. Early in 1959 he was appointed to the Ion Propulsion Committee of the American Rocket Society.

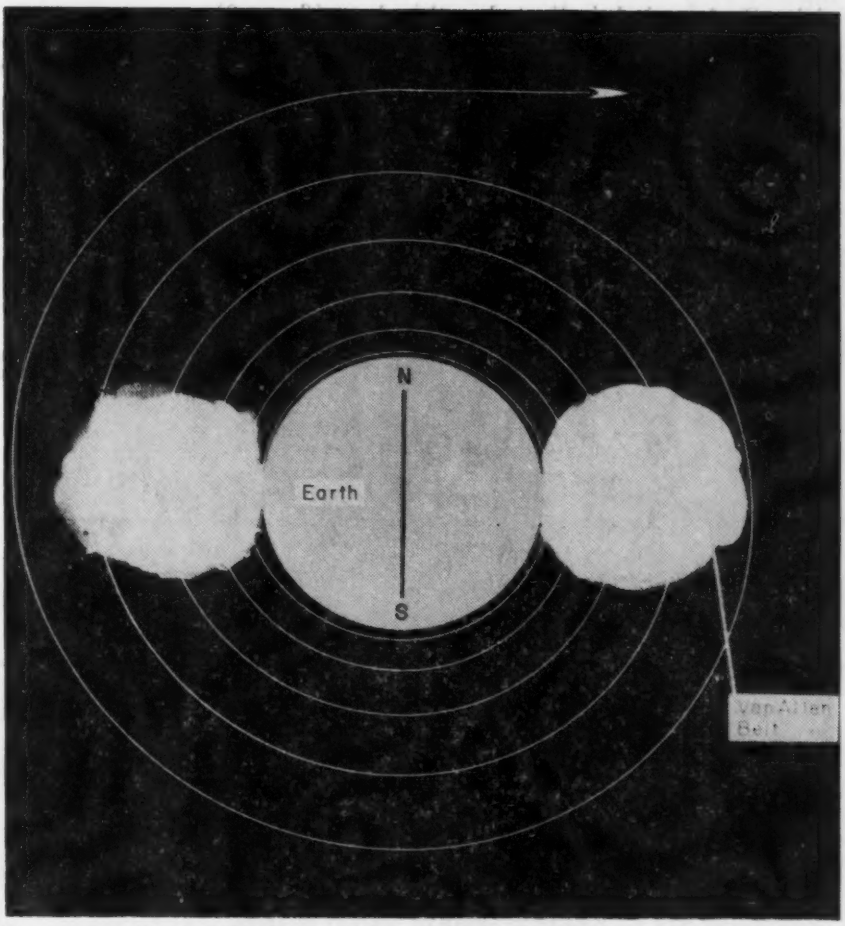


FIGURE 1.—Spiral path of electrically propelled vehicle leaving Earth's gravitational field.

Once a vehicle is at high altitude—say several thousand miles so that it is outside the Van Allen belt—then this problem no longer arises. Now, although the acceleration is taking a very long time, this is not very important for an interplanetary trip since such a trip is going to take 2 to 3 years anyway. Hence, it does not matter if we spend many weeks or even months of this time to achieve the necessary high velocity that will transport the vehicle to the planet we desire to visit. Such a trip is indicated on this chart (figure 2).

Here we have the vehicle accelerating from the earth's orbit, coasting, and going to Mars' orbit. If we were to start right back, we

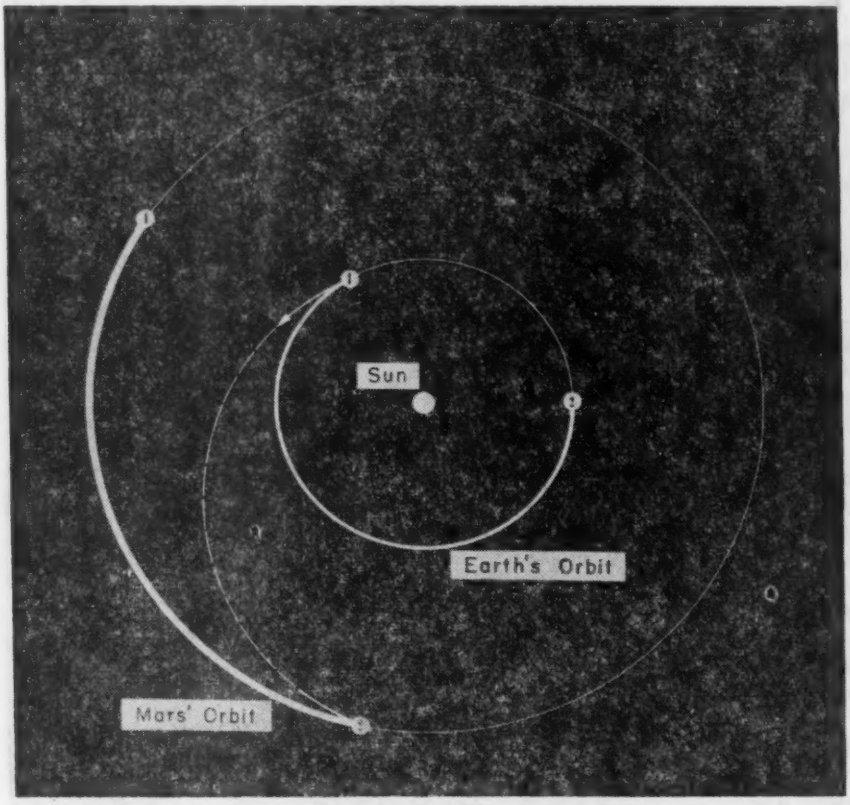


FIGURE 2.—Path of journey to Mars.

would find that when we got back to the Earth's orbit the Earth would be on the other side of the sun from us. So we have to wait something like a year or so in Mars' orbit anyway before we can come back.

So, in the long run, the fact that the acceleration is a factor of 10,000 less than that of chemical rockets is not very important.

It is important to point out, however, that although an electric rocket can go much faster than a chemical rocket, there is an upper limit to the final velocity of the rocket itself in spite of the fact that the exhaust velocity can be as high as we wish. The reason is simply that the total amount of energy that we can extract from the nuclear reactor—which is our prime energy source—is limited. Eventually,

the fuel elements will crumble, or something of this sort will happen. For example, we could not think of making a trip to the planet Pluto and getting back in 3 years. The velocity that would be required to makes this possible is simply too high to be achieved with the kind of reactors which we know how to build today. Trips to the stars are completely out of the question. Even using the most optimistic estimates of the performance of these vehicles, the time required for such a trip would exceed human lifespan.

Now there are two basic problem areas that we must consider. First, we require a very lightweight electrical powerplant which can operate in space. Studies that have been done at the Lawrence Radiation Laboratory and elsewhere have shown that the plant should not weigh more than about 5 to 10 pounds for each kilowatt of electric power produced. This is about a factor of three heavier than an automobile engine, for example. Secondly, we must have an efficient way of using this electricity for accelerating the fuel to very high velocities.

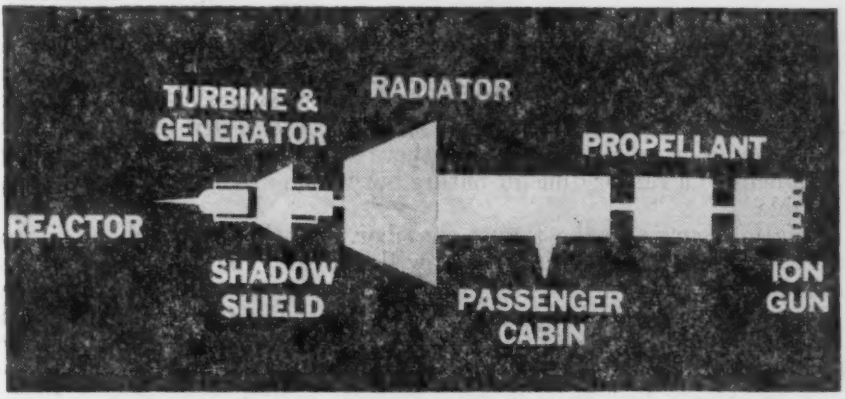


FIGURE 3.—Schematic diagram of electric propulsion system.

Now let's take a brief look at the system as a whole and try to see the nature of the problems involved. I have here a schematic diagram of an electric propulsion system (figure 3). At the beginning here we see the reactor together with the shielding to protect humans if we are carrying them, and the main powerplant and electrical generator; also the radiator for rejecting waste heat. These elements comprise the main electric powerplant.

There are a pair of basic problems whose solution determines the weight of this system. First is the fact that we are generating our energy in the form of heat in this reactor, and we are asking that it be transformed into electrical power. There is a very basic law of physics which says that to do this we must throw away some of the energy in the form of heat. This is a basic requirement which we cannot avoid or get around by any trick.

To reduce the amount of heat or energy that we reject in this manner requires that the heat be produced at very high temperature and that the waste heat be rejected at low temperature.

Unfortunately—and this is the second problem—we are talking about having this vehicle out in space; and the only way we can reject

this heat in space is by radiating it from a hot surface. This is a very inefficient way of eliminating heat and requires a great deal of surface and, hence, weight, unless the temperature is high. So we see here a contradiction.

The resulting problem has been studied in some detail, and the result is that to get the necessary light weight of the system, we should have a reactor temperature in the neighborhood of 2,000° F.—this is white hot—and the radiator temperature should be something like 1,000° F., a dull red heat.

The powerplant as a whole is basically no different than a large nuclear powerplant such as the PWR, except that no water is available for carrying away the waste heat. Because of the resulting very high temperatures in the system, we can no longer use a steam plant for converting the heat to electricity, but, rather, must replace the water and steam by a liquid metal such as potassium or rubidium and its vapor.

Associated with this very high temperature, however, are very serious problems of chemical corrosion, and these corrosion rates will eventually determine how high this temperature can actually be. This is the basic and probably the most fundamental problem connected with achieving this very lightweight electrical powerplant. What is needed here is a long-range basic research activity in corrosion at high temperatures. Of course, there are many other engineering problems of a rather difficult nature but which are basically, we know, soluble.

We now come to the thrust chamber. In the thrust chamber we take the propellent fluid which will probably be cesium metal—a rather odd metal which is liquid at room temperatures—and if we are going to work with the atoms, we will first ionize the metal. By this we mean remove one of the electrons from each atom. Then we may either separate the electrons from the ions, accelerate the two particles separately by means of high voltage, and then recombine them after the proper velocity has been reached; or we may form what is called a plasma, a mixture of the two particles together, and accelerate them by means of an electromagnet.

Both methods are being considered, and research is being done on them in various parts of the country. It is really too early to tell which will prove to be the better technique.

Actually, the size of the particle that we accelerate does not really matter. We can accelerate ionized dust particles, for example, just as easily as individual atoms; and, in fact, some consideration is being given at certain laboratories to the feasibility of doing exactly this. In any case, all these schemes require that we either form this plasma or that we separate the ions or dust particles and the electrons. There is an appreciable amount of energy used up in these processes, and this sets an upper limit to the efficiency.

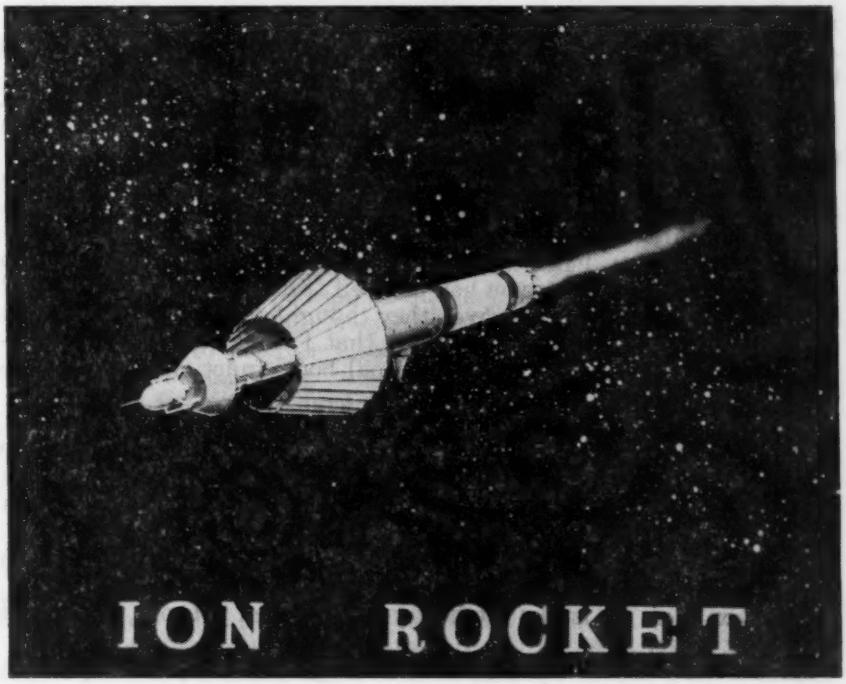
These problems are being studied. There seems now to be every reason to expect that we can achieve at least 50 percent efficiency in these devices and some hope that we can do much better—say 80 or 90 percent. Much more basic physics research needs to be done on both electric and magnetic acceleration before we can hope to do this, however.

Finally, it might be well to give you a more pictorial idea of an electric rocket and an idea of the size of the thing we are talking about. So I have here a pictorial representation of such a rocket (figure 4).

We see here the reactor, the shield, the turbomachinery and generator, this very large radiator. You see the amount of area. In fact, this is probably very optimistic. And here is the cabin where we might be carrying people, and, finally, the fuel tank and the thrust chamber. Remember that the entire device, except for the cabin and fuel tank, is at red heat.

To carry two or three persons, say on a round trip to Mars, would probably require that the cabin and the supplies for these passengers

FIGURE 4



and their equipment weigh something like 30,000 pounds. And in this case we find that we need something like 50,000 pounds of fuel, which is probably liquid cesium, and that the gross weight of the vehicle be about 100,000 pounds.

Now this thrust chamber which you see at the back end, to push this through space toward Mars and to bring it back, will put out a thrust of something like 15 pounds, which you see is really not very much. The power consumption in this thrust chamber is something like 1,000 kilowatts. This is about four times the power output of one of our larger automobile engines.

It should be clear from this discussion that there are major technical problems to be solved and that much basic long-range research is required if we are to achieve the kind of performance that is necessary for carrying humans among the planets. A rough estimate of

the time scale required before such a device might actually come into being is probably something like 15 to 20 years, possibly as high as 30 years. Devices of this kind are not around the corner. Right now we are about in the same position as we were with respect to jet engines in the late 1930's.

Mr. PRICE. Dr. Fox, you say ion propulsion systems have very low thrust compared to chemical rockets.

Dr. FOX. That is low thrust to weight ratio.

Mr. PRICE. Why?

Dr. FOX. The reason is that basically we have to convert this heat energy into electrical energy, and this requires a very heavy system. The chemical systems convert heat energy directly into kinetic energy of the exhaust gases in the nozzle. This is a process that can be carried out much more easily.

Mr. PRICE. Is there a physical law of nature that prevents high thrust by ion propulsion?

Dr. FOX. Not one; there is a combination of several laws, one that requires that we reject heat to get the electrical energy, and this in turn requires very large areas.

Senator ANDERSON. I might say I think it is a very interesting paper. I wanted to ask you, on page 3 where you say that it takes a long time to travel around, and that trips to the stars are completely out of the question, I notice that some scientists a while ago came up with the theory that once you had escaped from the gravitational pull of the earth and started to travel, they thought pretty rapidly toward the planet, you might get into a state that I can't describe—hibernation at least, timelessness, where normal body functions would cease to exist.

Is that part of the theory of this at all?

Dr. FOX. I suspect what he had reference to was the so-called time contraction that results from the theory of relativity, if one is going close to the velocity of light. However, this is not possible with a nuclear energy source. One cannot go that fast.

Mr. HOSMER. You spoke of the necessity of rejecting heat from these radiators, that goes off like heat waves.

Dr. FOX. Yes. It is like the heat you feel when you put your hand near a hot stove.

Mr. HOSMER. Is there any counterthrust generated by the departure of these heat waves?

Dr. FOX. There is. You can calculate it, and it is completely negligible.

Mr. HOSMER. You haven't mentioned what the speeds are that you visualize in connection with this thing.

Dr. FOX. Speeds like 25 miles per second.

Mr. HOSMER. That is not very fast.

Dr. FOX. That is fast enough for most interplanetary travel. Fifty miles per second seems to be getting very difficult to achieve.

Mr. HOSMER. You are constantly accelerating with this device; is that right?

Dr. FOX. That depends on the mission. You might or might not be. If you are going Mars and want to do it right, you would be a little more sophisticated than I indicated on the chart, and you would be accelerating or decelerating all the time. On the trip to Jupiter you

might achieve the speed you require much earlier in the game, and then just coast.

Mr. PRICE. Thank you very much, Dr. Fox, for a fine and interesting paper.

Dr. FOX. Thank you, sir.

Mr. PRICE. The next witness will be Dr. Robert E. English of the National Aeronautics and Space Administration.

STATEMENT OF ROBERT E. ENGLISH,[1] NATIONAL AERONAUTICS AND SPACE ADMINISTRATION, LEWIS RESEARCH CENTER

Mr. ENGLISH. Thank you, Mr. Chairman.

Mr. PRICE. You may proceed, Doctor.

Mr. ENGLISH. Gentlemen, it pleases me to have the chance to discuss nuclear energy for space applications. I am pleased that there is so much interest in it. NASA is one of those groups that also feel it is a very promising area. We would like to see this work advanced as well.

Inasmuch as the time allotted to this discussion is relatively short, only a small portion of NASA's program can be described. For this reason I have chosen to discuss application of a single device, SNAP 8. SNAP 8 was selected for discussion because of its attractiveness for some missions and because I thought it would be of interest to you.

This is an early application of the very kind of thing that Dr. Fox was describing, the use of a nuclear reactor in combination with a conversion system to produce electric power which would, in turn, be used for propulsion.

NASA and the AEC have agreed to jointly develop the SNAP 8 power supply, and it should provide the first useful application of electric proplusion to space missions. Its schematic arrangement is shown in figure 1.

The AEC will provide the reactor, and NASA the machinery for converting the reactor's heat to electric power. The exact form of the powerplant is not yet well defined because NASA is just now selecting a contractor for development of the conversion machinery, but some general characteristics of the device can be described.

Heat from the reactor will be used to boil a liquid metal, probably mercury. The resulting vapor will be passed through a turbine that is used to drive an electric generator. The vapor leaving the turbine will be condensed in the radiator, and the resulting liquid will then be pumped back into the boiler.

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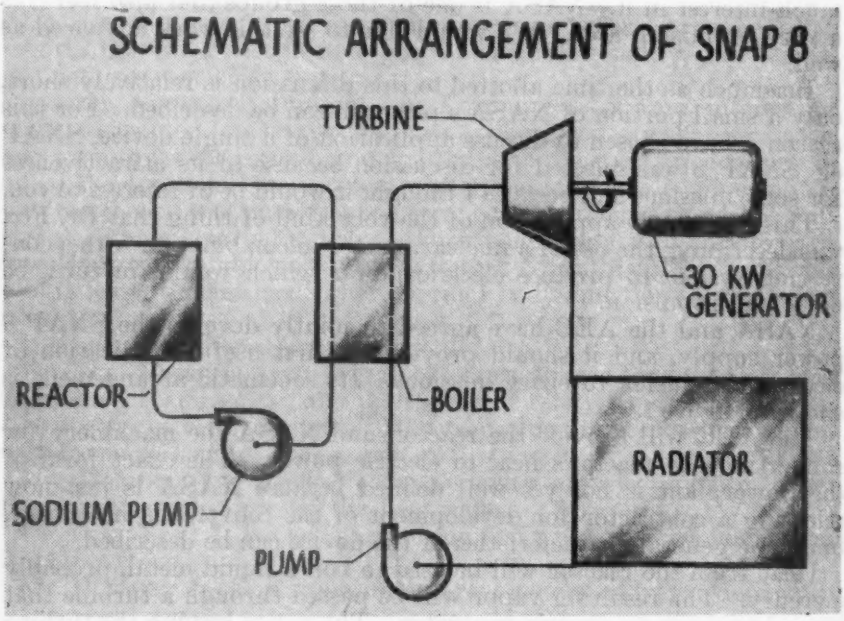
[1] Born in Grand Rapids, Mich., in 1920. He was raised and educated in Minneapolis and St. Paul, Minn. He obtained his technical education at the University of Minnesota, which granted him both a bachelor of mechanical engineering degree and a master of science in mechanical engineering. After working in research for 1 year at the University of Minnesota, he taught in the department of mechanical engineering for 2 years. Sixteen years ago he joined the National Advisory Committee for Aeronautics; after 2 years he was made head of a turbine aerodynamics research section. He is currently Associate Chief of the Power Production and Shielding Branch of the National Aeronautics and Space Administration.

He is the author of various reports and papers on combustion chambers, turbines, gas properties, turboprop engines, turbojet engines, and nuclear turboelectric power supplies for use in space. The Atomic Energy Commission, the Department of Defense, and the National Aeronautics and Space Administration are currently investigating the potential hazards of nuclear space systems, and Mr. English is Chairman of the Inter-Agency Committee formed for this purpose. He lives on the outskirts of Cleveland, Ohio, with his wife and three daughters.

The electric power output of the generator will be 30 kilowatts. The reactor, however, will have the capacity to provide enough heat for production for 60 kilowatts of electric power. For this reason, SNAP 8 will also have a 60-kilowatt version that will employ two sets of conversion machinery and a single reactor. These two power levels will given greater flexibility in application of SNAP 8 than would just a single power level. For example, the 30-kilowatt version will be suitable for use with Agena-launched space vehicles, and the 60-kilowatt version with Centaur-launched vehicles. The 60-kilowatt model will weigh about 3,000 pounds, or 50 pounds per kilowatt. This is a factor of 5 to 10 higher than the goal Dr. Fox indicated we should strive for.

As Dr. Fox indicated, these electric-propulsion systems have to be put into space by some launching vehicle. Anticipated performances

FIGURE 1



of space vehicles that use SNAP 8 for electric propulsion are shown in figures 2 and 3. In each case the 60-kilowatt version of SNAP 8 was assumed to be launched into an orbit that is 300 miles above the earth. The satellite in this orbit was taken to weigh 9,000 pounds, about the capacity of the Centaur launching vehicle. After a stable orbit about the earth has been attained, the reactor and the conversion machinery would be started. This procedure was selected as being a safe way to use nuclear power for this application. The electric power would be supplied to an electric rocket for propulsion of the space vehicle to higher and higher orbits about the earth. This orbit lies around 22,000 miles above the earth's surface, and at this altitude the space vehicle would circumnavigate the earth in a period of 24 hours. If it were observed from the surface of the earth, it would appear to stand still. This is an attractive means for worldwide communication. Three

FIGURE 2

NASA
C-53020

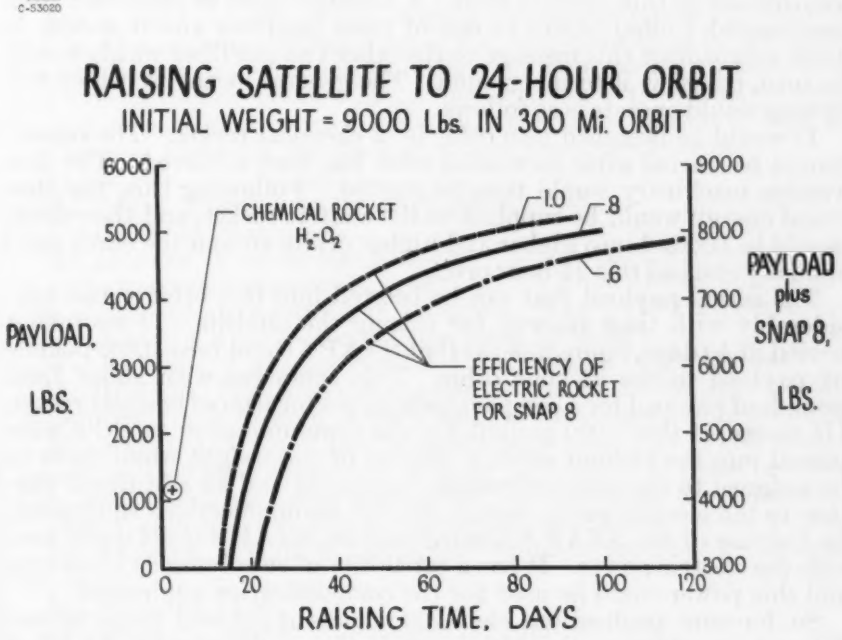


FIGURE 3

NASA
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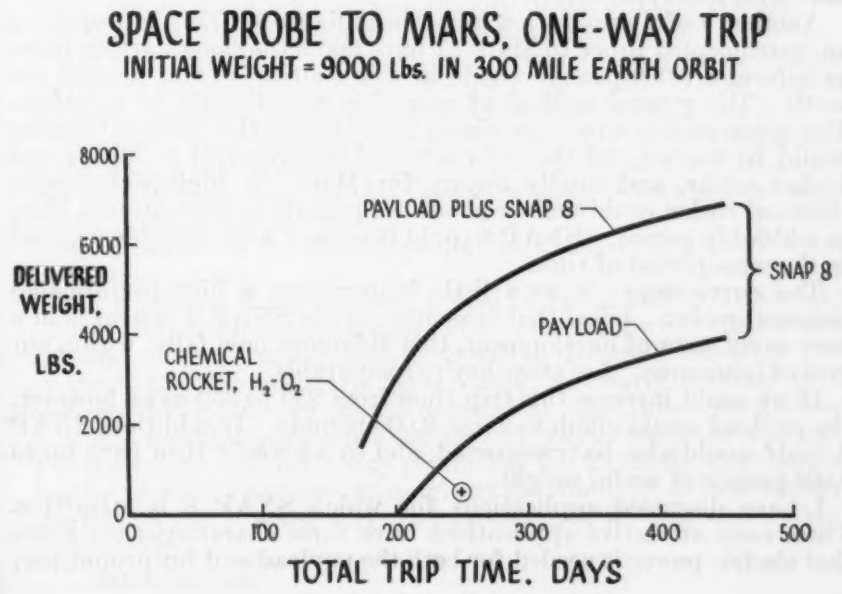


satellites could be placed in the orbit, equally spaced around the circumference of this circular orbit. A message could be broadcast from continental United States to one of these satellites and it would, in turn, rebroadcast this message to the other two satellites which would, in turn, transmit it to the ground. The general means by which this system would operate is as follows.

It would be launched into orbit by a chemical rocket. The reactor would be started after successful orbit has been achieved. The conversion machinery would then be started. Following this, the electrical energy would be supplied to the electric rocket, and the vehicle would be boosted into higher and higher orbits around the earth until it finally reached this 24-hour orbit.

The actual payload that can be boosted into this orbit varies considerably with time allowed for raising the satellite. If we take a period of 40 days, figure 2 shows that SNAP 8 could raise 3,000 pounds of payload to the 24-hour orbit. This compares with about 1,000 pounds of payload for a comparable high performance chemical rocket. Of course, if this 1,000 pounds for the communication satellite were placed into the 24-hour orbit, a portion of the weight would have to be assigned to the communications equipment and an additional portion to the electric power supply for the communications equipment. In the case of the SNAP 8-boosted vehicle, SNAP 8 itself could provide the electric power. It has a capability of supplying 60 kilowatts, and this power could be used for the communication equipment.

So, for some applications, the 3,000 pounds of payload would be useful and, in addition, the 3,000 pounds that a 60-kilowatt SNAP 8 weighs would also be useful.

So I have shown at the right of figure 2 a scale of the weight of payload plus the weight of SNAP 8. If you didn't need as much as 60 kilowatts, the useful weight would lie somewhere between the 3,000 and 6,000 pounds. In either event, SNAP 8 would be a very useful device for this application.

Another useful application is shown in figure 3. This is propelling an instrumented probe to Mars. I have taken the same starting point as before, a 9,000-pound vehicle in a low-altitude orbit around the earth. The general method of operation would again be as before. The space vehicle would be placed in orbit and the electrical system would be started and the vehicle would be propelled to higher and higher orbits, and finally depart for Mars. A high-performance chemical rocket could deliver about 500 pounds to orbit around Mars in a 250-day period. SNAP 8 could transport a comparable payload in the same period of time.

The curve shows it as a little higher than a high-performance chemical rocket. I feel that inasmuch as the SNAP 8 system is in a very early state of development, this difference now falls within our area of ignorance. Let's say they're comparable.

If we could increase this trip time from 250 to 350 days, however, the payload would climb to about 3,000 pounds. In addition, SNAP 8 itself would also be transported, and so we would then have up to 6,000 pounds of useful weight.

I have discussed applications for which SNAP 8 is attractive. These most attractive applications have three characteristics: First, that electric power is needed for both the payload and for propulsion;

the second characteristic is that a large amount of propulsion is required; the third is that the total mission should last for at least a month in order that the slowly starting electric propulsion system will not be at a serious disadvantage. We anticipate that future space vehicles employing electric propulsion can be expected to have even better performance because larger powerplants will themselves yield improved performance and additional design information will be available at that time for designing these improved systems.

Thank you.

Mr. PRICE. Thank you very much, Dr. English.

Are there any questions?

Mr. HOSMER. I would like to know why you have a secondary circuit in your steam generation system. This whole system will be outside someplace anyway and unavailable. Why go to the refinements that you have in a nuclear propulsion plant for ships?

Mr. ENGLISH. I am not certain I understand your question.

Mr. HOSMER. Why don't you generate the steam for your turbine in your reactor and run it directly and cut out your secondary circuit?

Mr. ENGLISH. I think this is basically a good idea. There are a couple of problems associated with it. I think it is these problems that prevented their being used in such an immediate development.

(Mr. Wetch added to this in the beginning of his testimony, p. 300.)

Mr. HOSMER. Weight is the biggest problem you have in the space business.

Mr. ENGLISH. Yes.

Mr. HOSMER. You have 50 pounds per kilowatt here.

Mr. ENGLISH. Yes. The thing that you would like to take out is the boiler, which might weigh of the order of 100 pounds. This would help the 3,000 pounds a little but not a great deal.

Mr. HOSMER. You have two sets of coolants, the primary and the secondary plus all your hardware and plumbing.

Mr. ENGLISH. That is right.

Mr. PRICE. Wouldn't the radioactivity in the primary coolant, have something to do perhaps with your possessing a second circuit?

Mr. ENGLISH. This is one of the problems. Another is that mercury is not a satisfactory coolant for use in the reactor. It swallows up too many neutrons and thus makes the reactor design difficult.

The thing you described, the possibility of boiling in the reactor, is something that is considered for this application, but it is not sufficiently well understood at this time to be suitable for use in as early a device as this is intended to be.

Mr. HOSMER. What advantage does this have over a direct conversion system, if any?

Mr. ENGLISH. It is, in effect, a bird in hand, and it is this which gives it the principal advantage. There is a variety of types of direct conversions, and I understand these will be discussed tomorrow. One of these, the thermoelectric type, would be clearly inferior to this turboelectric system, in that it operates at much lower temperatures and thus would require much larger and heavier radiator equipment. The thermionic emitter is very promising for future development, and it is being widely explored throughout the country. It is a device for the future, and it is too early to be considered for this application as we intend it.

56108—60——20

Mr. HOSMER. It is certain that in devices like that you can float them out away from the main vehicles while in space and have the wire strung out to them and eliminate the shielding problem.

Mr. ENGLISH. I don't understand that in this way.

Mr. HOSMER. Instead of one of these, have 10 thermoelectric devices.

Mr. ENGLISH. What is the energy supply?

Mr. HOSMER. And have them out around and ahead of the space vehicle itself.

Mr. ENGLISH. What is the energy supply for these? If they were to be used in combination with nuclear reactors they would necessarily be near the nuclear reactor.

Mr. HOSMER. Say you have long-lived heat-producing isotopes.

Mr. ENGLISH. These would not be suitable. They are far too heavy and there is not an adequate supply of radioisotopes available. It would be impractical to produce enough radioisotopes to do this job. This is the kind of thing I am confident the people in the AEC would agree with me on. We work very closely in this area.

This general problem has come up for considerable amount of discussion and exploration, the very kind of thing you bring up. I think we and they both agree that this is a good way to approach this problem.

Mr. HOSMER. I see that a part of your work has to do with shielding.

Mr. ENGLISH. Yes.

Mr. HOSMER. Do you do that yourself in NASA or does the AEC do it for you, or do you both do it?

Mr. ENGLISH. There is not a clear answer to your question. I think that we can take the way that this particular device —SNAP 8—will be produced as the way that the bulk of our work would be approached. We have asked the AEC to do the nuclear phase of it, and they are doing it to our exclusion. We think that this is as it should be.

We think we should also be knowledgeable in the area of nuclear energy. The right hand should know what the left hand is doing. It is not a practical thing to develop one phase of this system quite independent of another, and then, later on, to fit these pieces together and attempt to assemble them all into a vehicle and to do a useful job with them. There must be some integration of each component into the whole, and the whole device, such as this, into a vehicle.

Mr. HOSMER. My question was really directed to whether the right hand and left hand were both doing the same things. I think you indicated they are not. Is that correct?

Mr. ENGLISH. Yes, that is right.

Mr. HOSMER. Thank you.

Mr. PRICE. Thank you very much, Mr. English.

Mr. ENGLISH. Thank you.

(The full statement of Mr. English follows:)

POTENTIAL APPLICATIONS OF SNAP 8 TO SPACE MISSIONS

By Robert E. English, National Aeronautics and Space Administration, Lewis Research Center

Because the time allotted to this discussion is relatively short, only a small portion of of NASA's program can be described. For this reason, I have chosen to discuss application of a single device, SNAP 8. SNAP 8 was selected for discussion because of its attractiveness for some missions and because I thought it would be of interest to you.

NASA and the AEC have agreed to jointly develop the SNAP–8 power supply, and it should provide the first useful application of electric propulsion to space missions. Its schematic arrangement is shown in figure 1 (p. 294). The AEC will provide the reactor and NASA the machinery for converting the reactor's heat to electric power. The exact form of the powerplant is not yet well defined because NASA is just now selecting a contractor for development of the conversion machinery, but some general characteristics of the device can be described. Heat from the reactor will be used to boil a liquid metal, probably mercury. The resulting vapor will be passed through a turbine that is used to drive an electric generator. The vapor leaving the turbine will be condensed in the radiator, and the resulting liquid will then be pumped back into the boiler.

The electric power output of the generator will be 30 kilowatts. The reactor, however, will have the capacity to provide enough heat for production of 60 kilowatts of electric power. For this reason, SNAP 8 will also have a 60-kilowatt version that will employ two sets of conversion machinery and a single reactor. These two power levels will give greater flexibility in application of SNAP 8 than would just a single power level. For example, the 30-kilowatt version will be suitable for use with Agena-launched space vehicles, and the 60-kilowatt version with Centaur-launched vehicles. The 60-kilowatt model will weigh about 3,000 pounds, or 50 pounds per kilowatt.

Anticipated performances of space vehicles that use SNAP 8 for electric propulsion are shown in figures 2 and 3 (p. 295). In each case, the 60-kilowatt version of SNAP 8 was assumed to be launched into an orbit that is 300 miles above the Earth. The satellite in this orbit was taken to weigh 9,000 pounds, about the capacity of the Centaur launching vehicle. After a stable orbit about the Earth has been attained, the reactor and the conversion machinery would be started. This procedure was selected as being a safe way to use nuclear power for this application. The electric power would be supplied to an electric rocket for propulsion of the space vehicle to higher and higher orbits about the Earth.

Figure 2 shows the weights that could be boosted by this process into a 24-hour orbit. The 24-hour orbit is about 22,000 miles above the Earth's surface, and a satellite in this orbit would take just 24 hours in circumnavigating the Earth. A combination of three such satellites would be useful in a worldwide communication system in which signals from the ground would be received by one satellite, would be broadcast from that satellite to another, and would then be rebroadcast to the ground.

Three efficiencies for the electric rocket are given in figure 2 because the actual efficiency for the required operating conditions cannot yet be very well estimated in this case. The weight that SNAP 8 could raise to this orbit varies considerably with the time permitted for the raising. By propelling for about 40 days, the electric propulsion system could raise a payload of 3,000 pounds to the 24-hour orbit.

A high-performance chemical rocket can place about 1,000 pounds of payload into a 24-hour orbit in the plane of the Equator. For a communication satellite, part of this 1,000 pounds would be assigned to communications equipment and part to the required electric power supply. The satellite raised by SNAP 8 would, on the other hand, have the SNAP–8 power supply in addition to its other payload. For this reason, a second scale of weights is shown at the right side of figure 2. This scale shows the combined weight of payload plus SNAP 8 that would be raised into a 24-hour orbit. If the entire output of SNAP 8 is needed for the communications equipment, all of this weight can be classed as useful. If, say, only one-quarter of the 60 kilowatts is needed for communication, then only one-quarter of the weight of SNAP 8 should be taken as a useful addition to payload. On either basis, SNAP 8 is a very useful device for such mission.

Another useful application is propelling an instrumented probe to Mars, for which estimated weights are given in figure 3. For both a chemical rocket and a SNAP 8-propelled space vehicle, payload varies with the desired trip time. If the space vehicle is accelerated to a high speed in order to achieve a low transit time, the permissible payload is reduced. A high-performance chemical rocket could deliver about 500 pounds of payload to orbit about Mars in 250 days. SNAP 8 could transport a comparable payload in the same period of time. But if the trip time were increased to 350 days, the payload could be 3,000 pounds. In addition to this payload, SNAP 8's 3,000 pounds would also

arrive at the destination, and 60 kilowatts of electric power would be available for use by the payload.

Those applications for which SNAP 8 will be especially attractive involve several factors: (1) Electric power is needed for both the payload and for propulsion. (2) A large amount of propulsion is required. And (3) the total mission will last for at least a month so that the slowly starting electric propulsion system will not be at a disadvantage. Future space vehicles employing electric propulsion can be expected to provide performance superior to that with SNAP 8 both because larger powerplants will yield improved performance and because additional design information will be available at that time.

Mr. PRICE. The final witness for this afternoon will be Mr. J. R. Wetch of Atomics International.

STATEMENT OF J. R. WETCH,[1] CHIEF PROJECT ENGINEER, SNAP REACTOR PROGRAM, ATOMICS INTERNATIONAL DIVISION, NORTH AMERICAN AVIATION, INC.

Mr. WETCH. Thank you, Mr. Chairman.

Mr. PRICE. You may proceed, sir.

Mr. WETCH. My discussion this afternoon will introduce and, in general, make some comments about the SNAP program which you have heard a little bit about today, and then I would like to digress into some of the things that are a little more along the lines that Dr. Fox and Dr. Ehricke were alluding to before.

Mr. PRICE. Perhaps you could start out by answering the question that Mr. Hosmer raised.

Mr. WETCH. With regard to the question as to why we use mercury as a turbine fluid and why we don't boil it in the reactor, the reason for that is that the current fuel technology limits us to about 1,200° to 1,300° F. Because we are attempting in the SNAP program at present to utilize the hydride alloy fuels so as to have a very low critical mass and, therefore, a relatively inexpensive nuclear reactor. In addition, we must be able to burn it up entirely on reentry so that there will be no public hazard with regard to radioactivity and the hydrides have that property. In attaining that property we must pay for it by means of a temperature limitation. This limitation is of the order of 1,200° to 1,300° F. That is a dull cherry-red, maybe like the end of your cigarette. The temperature is therefore limited there, and the only working fluid we know of that would today give us adequate efficiency to generate the power is mercury. There are a few others—sulfur, phosphorus, what-have-you, but they are far too

[1] Deputy Director of the Compact Power Systems Department at Atomics International, a division of North American Aviation, Inc. The compact power systems department is responsible for the development of the SNAP reactor program for the Atomic Energy Commission.

Prior to his present position, Mr. Wetch was chief project engineer for the SNAP project and headed the development of SNAP 2 since its inception at Atomics International. SNAP 2 (systems for nuclear auxiliary power) is a nuclear system to provide electrical power for use in space vehicles.

A native of Santa Rosa, Calif., he joined North American Aviation in 1952 as a research engineer in the atomic energy department. He was associated with the original design and optimization of the K production reactors while with the General Electric Co. at Hanford, Wash., before joining North American.

He received a B.S. degree in chemical engineering from the University of California at Berkeley in 1951. He has done graduate work at the University of California at Los Angeles, the University of Southern California, and the University of Washington.

He is a member of the Nuclear Energy Processes Research Advisory Committee of the National Aeronautics and Space Administration.

Mr. Welch is a member of the Secondary Power Committee of the American Rocket Society. He is chairman of the Powerplant Subcommittee of the Antisubmarine Warfare Task Committee, National Security Industrial Association.

corrosive. Mercury is the only fluid which we have which has good thermodynamic properties in this temperature range. Mercury cannot be boiled in a hydride reactor because it is a neutron absorber; therefore, it would increase the size and weight of the reactor and make it extremely difficult to control.

In future systems, that Dr. Fox alluded to, where we want to go to 1,600°, 1,800°, and up to 2,000° F. in the turbine system, there we would use rubidium or potassium which have higher temperature boiling points which are ideal thermodynamically for turbines. These fluids, we expect will have much higher boiling heat transfer rates than does mercury and do have low neutron cross sections, and can be boiled directly in the core. In addition to that, we will use in those temperature ranges ceramic fuels such as the uranium carbides or uranium dioxide or possibly beryllium oxide-uranium dioxide. These fuels require, if it is going to be a compact, small reactor, that it be a fast reactor of high critical mass. Thus, because of the high energy neutron spectrum there would be little neutron absorption in the rubidium or potassium. Consequently, the control problem would not be prohibitively difficult nor would coolant activation pose a limiting problem. Because of difficulty of reentry burnup these reactors may be restricted to high-altitude orbits and for much more ambitious missions. They will be expensive. Consequently, they are less desirable for the low-altitude auxiliary power units. For electric propulsion they will be used.

Mr. PRICE. Thank you.

Mr. WETCH. The frontiers of atomic energy research and development are being pioneered and settled at a rapid rate in order to meet the challenge of the advancing frontiers of space. In the past few years, during our association with the AEC Office of Aircraft Reactors, Systems for Nuclear Auxiliary Power program, we have encountered a forest of technical obstacles and unknowns which were not fully appreciated at the program outset. When the program was initiated at Atomics International in 1955, some were even skeptical of the possibility of launching satellites and of making small nuclear power reactors. Of particular concern was the ability to convert reactor heat to electricity for the required long periods of time, unattended, at high temperatures, in the high-vacuum, weightless environment of outer space. Much of the technology and many of the materials such as metal hydrides, rare earth metal compounds, advanced propellants and semiconductors were laboratory curiosities of scientists who at that time were not necessarily involved with space exploration or compact reactor development. These technical curiosities have since been transformed into successful demonstrations of working hardware which indicates that space satellites, small nuclear reactors and small long-life power conversion machinery are possible and can perform as predicted.

The rapid progress in rocket booster development and in space satellite accomplishments is well known. We may therefore turn our attention to the nuclear aspects of space utilization.

The AEC's SNAP 2 project is the pioneer and forerunner of the future nuclear-electric space programs. Its success and progress will therefore greatly influence current and future thinking on the subject. Thus, a brief review of this program may be of interest.

The small 220-pound SNAP 2 reactor whose operation was announced by Mr. John McCone in November of last year is still performing at design conditions. To date it has produced the energy required to replace nearly 100,000 pounds of batteries at a continuous rate that would require about 1,000 square feet of solar cells which would cost about $3 million, or about 5 to 10 times the projected cost of the equivalent nuclear power unit.

A second major accomplishment is the operation of small mercury vapor turbine-generator devices in the AEC's SNAP program for over 1,000 hours, which might be compared to over 50,000 miles of continuous nonstop automobile operation. At this writing this test is still underway and no performance deterioration has yet been observed.

These demonstrations of feasibility by no means indicate that the job is done. Rather, we have only completed phase I, the research and feasibility phase of the SNAP 2 program. The larger effort of developing this hardware for the space environment, and integrating the power conversion machinery with the nuclear heat source and finally the satellite vehicle, is only now commencing. These forthcoming integration tests, automatic control tests, vibration and shock tests, and remote orbital startup demonstrations will require a great deal of attention over the next 2 to 3 years. Upon completion of these engineering development activities, a reliability and endurance qualification demonstration phase of at least a year will be required before these nuclear auxiliary power units would be operationally coupled with satellite vehicles which are valued in the millions and tens of millions of dollars.

Thus, nearly a decade will have passed since research and feasibility efforts were originally funded for development of a satellite nuclear APU, and the time that the nuclear APU will be ready for operational use.

Mr. PRICE. Our staff director would like to ask a question at this point.

Mr. RAMEY. What kind of reactor is the SNAP 2 reactor?

Mr. WETCH. This is what we call an epithermal reactor where the fuel and the moderator are homogeneously alloyed, and are cooled with liquid sodium-potassium alloy. This sodium-potassium alloy is liquid at room temperature. Then the liquid metal in SNAP 2, as indicated by Mr. English in the SNAP 8, is transferred out of the reactor into a heat exchanger, a boiler-superheater, where the mercury is boiled and superheated and then expanded through a turbine and finally condensed in the radiator, which is the method of heat rejection in space.

Mr. RAMEY. Is it the SNAP 1 program that uses the radioisotope?

Mr. WETCH. The SNAP 1 program was a radioisotope heat source, yes, sir.

Mr. PRICE. You may proceed.

Mr. WETCH. As a result of the progress in the SNAP 2 research, it has been possible to initiate development for the SNAP 10 program and the SNAP 8 program which may be considered as "peel offs" of the basic SNAP 2 research. They utilize the same advanced materials, physics, and control techniques, and operate in the same 1,200° to 1,300° F. temperature range. Thus, they differ only in power output and design details. The SNAP 10 system utilizes thermo-

electric "direct" conversion and delivers approximately one-tenth the SNAP 2 power output, or a few hundred watts. The SNAP 8 will utilize a scaled-up mercury turbine-generator conversion system which will deliver about 10 times the SNAP 2 power output, or tens of kilowatts. As a basic premise in the entire SNAP program, because our reactors are not power limited, we have made each of the reactors, so far, capable of twice the necessary heat output. So we may use two power conversion systems with any one of these SNAP reactors SNAP 2, SNAP 8, and SNAP 10, and double the power output in that manner. This was to give more flexibility to the entire space program.

Because these reactors are very similar, these programs can be launched nearly in parallel to the SNAP 2 program with a corresponding reduction in development time and cost.

During this decade of 1960 to 1970 these SNAP systems currently under development with demonstrated feasibility will become fully qualified and will be the predominant and most reliable source of high power that will be available for application in space satellites. Let us examine briefly what this means to our overall space capability.

Possibilities that exist with the SNAP 8 system with regard to electric propulsion have already been presented today. In addition to electric propulsion demonstrations and planetary probe auxiliary power, this family of nuclear powerplants can extend our current "exploration" of space activity to one of "utilization" of space within this decade. Nuclear auxiliary power coupled with current plans for launching Centaur and Saturn vehicles could provide this country with several global communications systems during the latter half of this decade. For instance, it is believed that these Earth satellite vehicles with their nuclear auxiliary power could provide reliable, low cost, all-weather, intercontinental telephone systems. This family of powerplants with these vehicles could also provide many channels of all-weather FM radio broadcasting coverage that could be received in all parts of the world regardless of the degree of local economic and broadcasting facility development.

It appears that the SNAP 8 system could provide sufficient power for one or more channels of continuous television broadcasting on a complete global basis that could be received by the conventional television sets now commonly in use. The family of SNAP units in this decade can provide sufficient electric power for near Earth satellites to accomplish precise air traffic control and navigational aid functions. Satellite global weather surveying and mapping can be much more detailed, reliable, and accurate when using the higher powers provided by SNAP units.

Today we are concerned not only with the immediate future of the 1960's but also the more distant future, the 1970's and beyond. The recounting of the SNAP development program indicates that at least a decade will have passed from the time a determination of a requirement was made and a research effort initiated until a useful space powerplant was produced. How then do we stand today when we attempt to appraise the 1970's?

It is evident that our vigorous national booster vehicle program will, with the Saturn, Nova, and nuclear Rover boosters, provide us with the enormous payload capabilities of tens and hundreds of thousands of pounds in near-Earth orbits.

What then, we may ask, will these payloads consist of?

Certainly, some will be space vehicles destined for our Moon, and later in the 1970's for our near planets, Mars and Venus.

Electric propulsion, a propulsion concept which has been discussed for your committee this afternoon, holds great promise for transporting to the Moon and even to neighboring planets a large fraction of each payload after it has been placed in near-Earth orbit.

Electric propulsion systems will require at least several hundred kilowatts and up to several thousand kilowatts of electric power to be compatible with our booster capability. The electric propulsion powerplants will ultimately require extremely high performance and temperatures and low weights for eventual-manned interplanetary travel in the 1970's.

In the meantime a several-hundred-kilowatt or a 1,000-kilowatt space electric powerplant system coupled with the current Atlas-Centaur and Saturn boosters would provide the capability for multichannel worldwide all-weather television broadcasting from "fixed" 24-hour orbit satellites that could reach all of the villages and peoples of the world equipped with low-cost TV receivers. High power high altitude nuclear powered satellites with high-frequency equipment currently appear to be the only way in which sufficient band width can be achieved which would permit serious consideration of economic television-telephone systems.

One thousand kilowatts of electric power would be sufficient to provide pressurization, air purification, communications power and experimental laboratory power, et cetera, required for at least a 10- to 20-man functional and "permanent" space station on high-altitude Earth orbits. Although lower power requirements are sometimes quoted for potential-manned Earth satellites of the exploration type, it should be noted that useful defense, surveillance, or communications vehicles will require large quantities of auxiliary power. For instance, the advanced aircraft weapons systems such as the high-altitude F–108 interceptor required about 100 kilowatts for a crew of two, and the B–70 weapons system requires up to 360 kilowatts for a crew of four. Thus, in a system as complex and as sophisticated as a large-manned Earth satellite with its increased requirements of long-term habitability and environmental control, it appears not unrealistic to think in terms of hundreds of kilowatts and as much as 1,000 kilowatts of auxiliary power.

The key requirements for all of the space vehicles and power systems that have been mentioned are endurance and reliability.

Because of the great distances of interplanetary travel, the electric propulsion plants must be capable of at least 1 year's endurance. Because of the cost of a worldwide satellite TV network or of large-manned space stations, it is imperative that their powerplant be capable of long endurance also. As power is increased, the size, cost, and importance of the vehicle in which it is installed will also increase. Consequently, risk of failures must be decreased. As a result, there will be an increased demand for demonstrated full endurance reliability. We can no longer expect 30 or 50 percent mission reliability, particularly when we are putting men out there.

Even an elementary statistical analysis will indicate that many of these nuclear power units must be life tested before the required reliability can be inferred. Therefore, the reliability and qualification testing program alone, if excess duplication of test facilities is to be avoided, must be stretched over at least a 3- to 5-year period of time. If only 5 years were allowed for the engineering development, then it is evident that we must undertake our first steps of determined research to establish design criteria immediately if we do not wish to seriously delay our total space capability.

Several very promising approaches currently exist for 1,000-kilowatt space nuclear power systems.

Due to the present incomplete state of knowledge in high-temperature materials and phenomena, and to the uncertainties in the feasibility of high temperature, long-life methods of power conversion, we are not today in a position to select the optimum concept for development. The selection of a single concept at this time would represent a substantial development risk. For instance, I believe the question prevails among our country's scientists as to whether 1,000-kilowatt systems should utilize high-temperature ceramic fuel technology with a boiling liquid metal-cooled reactor coupled with high-temperature turboelectric generator principles, or the less well understood but possibly more attractive thermionic, so-called direct conversion reactor and conversion principles which require even higher fuel temperatures. In fact, if only a high-powered reliable nuclear space powerplant were desired, the current SNAP turbogenerator design approach might be the most reasonable even though its weights at higher power might make it less attractive than either the direct boiling cooled reactor or the thermionic conversion reactor for some of the ultimate electric propulsion applications.

Whether only one or several concepts are ultimately developed for hundreds to thousands of kilowatts of power, we should immediately undertake several years of intensive, closely coordinated research aimed at each of these approaches. This research in materials and high-temperature phenomena applied to nuclear reactors and their associated power conversion systems, direct and mechanical, should be undertaken at a reasonable level before the final decision as to the best approach is made. This research will be one of the most valuable investments this country could make in technology. This technology will be utilized in many other applications of nuclear energy and high-temperature processing and power generation fields. In fact, almost the entire SNAP space program has some direct application to ground power systems.

In the meantime the current family of SNAP reactor powerplants will encounter and identify the unknowns in space and will demonstrate the hardware and techniques required for remote operation of nuclear powerplants in the reaches of outer space.

Mr. PRICE. Are there any questions?

Mr. HOSMER. No questions.

Mr. PRICE. Thank you very much, Mr. Wetch, for a very fine statement.

Mr. WETCH. Thank you, sir.

Mr. PRICE. I see Colonel Armstrong sitting over there. I wonder if he might bring us up to date on some facets of the SNAP program and the use of radioisotopes.

We don't want to take up much of your time, Colonel, but perhaps a brief comment would be helpful.

Mr. ARMSTRONG. You say you would like something on the radioisotope program?

Mr. PRICE. Yes. We would appreciate very much your bringing us up to date on it.

Mr. ARMSTRONG. Martin Co. plays a similar role to Mr. Wetch's organization, Atomics International, in that Martin Co. is our major contributor on the isotope heat sources as Atomics International is on our reactor heat sources.

As you know, the inpile operation of any reactor creates fission products. These gross fission products are a waste product to the primary source of the reactor. Such fission products have to be buried in desolate places to accomplish the proper waste disposal. By taking these waste products and separating out the very active, relatively long-lived isotopes, you have created an energy which you can use and which, in effect, you might say is a free energy. Some of these isotopes are the isotopes such as cerium, promethium, several of them with long half-lives. In other words, you can pick isotopes with half-lives of a few hours, a very few months, or a great many years, as you wish. By the simple process of applying this heat you can produce electricity. This has been the primary objective of our SNAP 1 and SNAP 1–A program of the Atomic Energy Commission. This was a device to create some 250 watts of electrical energy in a device which has no rotating machinery. This device is going to test shortly and will be placed on the shelf as an accomplished objective.

You are probably familiar, I am sure, with the SNAP 3 device, which was a proof-of-principle device which was shown the public some year and a half ago. This SNAP 3 device is still in our office. It is still producing electricity. The isotope has decayed down to the point where the amount of electricity being produced is something under half a watt.

We have several of these types of devices in the development process, using various isotopes and various means of converting this heat to electricity.

This is an active program and one for which we are greatly indebted to the Martin Co. for the development work.

I think, in a nutshell, Mr. Price, that is the isotope program.

Mr. PRICE. Thank you very much, Colonel. We appreciate your help on this matter.

Mr. ARMSTRONG. Yes, sir.

Mr. PRICE. That concludes the testimony for this afternoon. The committee will stand adjourned until 10 o'clock tomorrow morning, and the witnesses tomorrow will testify on direct energy conversion.

(For further information on nuclear space propulsion a description of magnetohydrodynamics and the potential application of thermonuclear reactions to rocket propulsion may be found in a report by Milton V. Clauser of Space Technology Laboratories, Los Angeles, Calif., Rept. No. PRL–8–04, June 2, 1958.)

(Whereupon, at 4:40 p.m., Thursday, March 24, 1960, the committee recessed until 10 a.m., Friday, March 25, 1960.)

FRONTIERS IN ATOMIC ENERGY RESEARCH

FRIDAY, MARCH 25, 1960

CONGRESS OF THE UNITED STATES,
SUBCOMMITTEE ON RESEARCH AND DEVELOPMENT,
JOINT COMMITTEE ON ATOMIC ENERGY,
Washington, D.C.

The subcommittee met at 10 a.m., pursuant to recess, in room P–63, the Capitol, Hon. Melvin Price (chairman of the subcommittee) presiding.

Present: Representatives Price, Hosmer, Westland, and Bates; Senator Aiken.

Also present: James T. Ramey, executive director; George F. Murphy, professional staff member, Joint Committee on Atomic Energy.

Representative PRICE. The committee will be in order.

This morning in our fifth frontier session we will cover direct energy conversion and solar energy systems.

We have with us scientists and engineers who are leaders in this advanced field of research. We look forward to hearing about the various advance energy sources in this field and for a comparison of the potentials of the various concepts.

The first witness this morning is Mr. Paul Egli, of the Office of Naval Research, who will first give us a broad picture of the concepts involved in this work.

We are pleased to have you with us, Mr. Egli.

STATEMENT OF PAUL H. EGLI,[1] HEAD, CRYSTAL BRANCH, NAVAL RESEARCH LABORATORY

Mr. EGLI. Thank you, Mr. Chairman.

Before starting my formal remarks, I would like to call attention to the devices that are on display. The one on my immediate right is an early model of a thermoelectric generator provided by the West-

[1] Mr. Egli attended Evansville College and Purdue University. Received B.S. degree in 1938 and M.S. degree in chemical engineering, 1940. With General Petroleum Division of Socony Mobile, 1940–41, and with Eli Lilly Co., Indianapolis, Ind., 1941–42. U.S. Navy, 1942–46. Since 1944 Mr. Egli has been head of Crystal Branch of the U.S. Naval Research Laboratory. In this position he has directed a program of materials development and solid state physics research. He has made significant contributions to the theory of crystallization and to the techniques for growth of a variety of single crystals. He is responsible for the development of many electronic materials in use today including piezoelectric crystals and ceramics used in submarine detection, optical materials used as elements in ultraviolet and infrared systems, photo detectors used for visible and infrared radiation, semiconductors for various purposes, and ferrites for microwave elements.

He has been active in the development of materials for energy conversion processes for a number of years and was the technical adviser to the U.S. Navy Bureau of Ships in establishing the Navy program on thermoelectricity. He is now acting in addition for the Power Branch of the Office of Naval Research as coordinator of direct energy conversion.

He is a fellow in the American Physical Society and the American Association for the Advancement of Science, is a member of the American Chemical Society and the Washington Philosophical Society, and has served on numerous Government and technical society committees.

inghouse Corp. (See photographs, pp. 309–310.) It produces 100 watts at about 10 volts and is able to operate the various devices on the table: a grinding wheel, automobile headlights, and an automobile type of radio-phonograph.

On my far right is a fuel cell provided by the National Carbon Co. which is operating on hydrogen and air.

It is a 6-volt cell at about 1 ampere output.

Representative PRICE. Where is the source of power for the gadgets you just displayed?

Mr. EGLI. A propane tank furnishes the fuel for a thermoelectric generator. There is a hydrogen cylinder in the bottom of the case for the fuel cell. I will describe the processes later in the presentation, if I may.

I should also like to comment, Mr. Chairman, that the gentlemen who are operating the charts are more than simply spearbearers in the presentation. Commander Kent Lee is one of the key personnel in the direct conversion program of the Navy and Mr. Harry Fox is a fuel cell expert in the Office of Naval Research. They are available for consultation if questions on their specialties should get beyond my depth.

Research on direct conversion is directed primarily toward utilization of nuclear energy. The same conversion processes, however, work equally well with other sources of energy, and for any particular application all possible combinations of energy source and conversion processes need to be examined.

We are concerned, therefore, with utilizing all three fundamental sources of energy:

1. Solar energy, both in the form of radiation and absorbed as heat;

2. Chemical energy, primarily as the combustion of fossil fuel, but also as electrochemical reactions; and

3. Nuclear energy producing heat from the decay of isotopes and from fission processes in reactors.

Before discussing the energy conversion processes, I think that in view of some of the questions that have arisen in previous testimony perhaps I should define direct conversion.

I intend to include in direct conversion any process which does not transfer energy to a working medium, a secondary fluid.

A combustion engine, for example, depends on the vapor working against the machinery. A turbine depends on steam or gas.

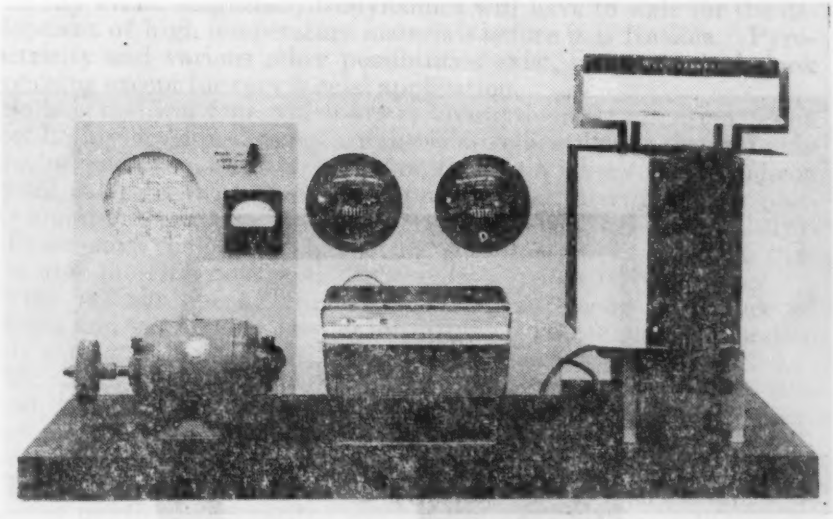
Those we would not regard as direct conversion. This definition and its interpretation can become a matter of semantics and I will concede that the direct collection of charges from nuclear processes would be the most direct.

We have given that possibility a tremendous amount of attention. We have examined carefully many proposals that involve direct collection of charges and at the present time none of them look even remotely promising.

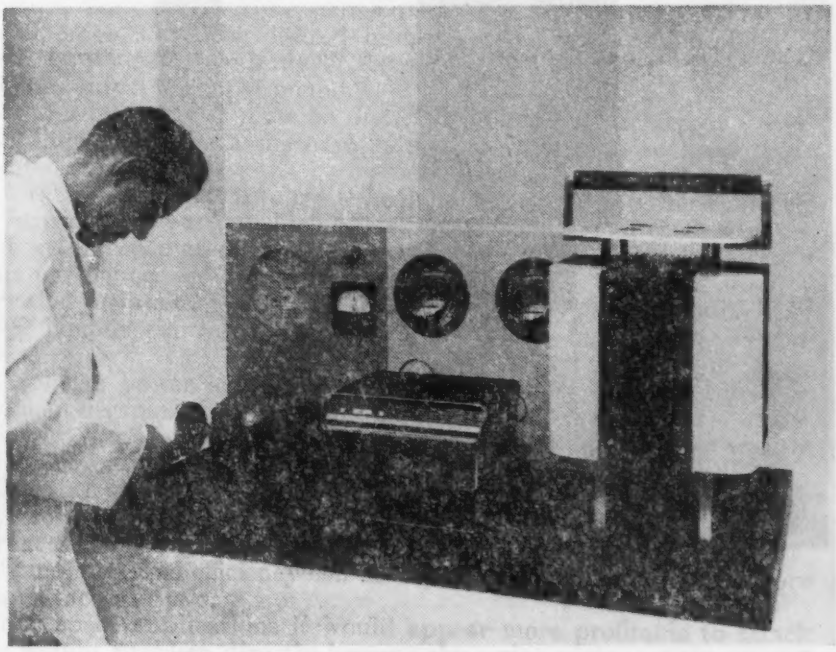
So the conversion processes with which we are concerned are primarily those involving solar cells, thermoelectricity, thermionic diodes, and fuel cells.

There are a number of others that can be listed.

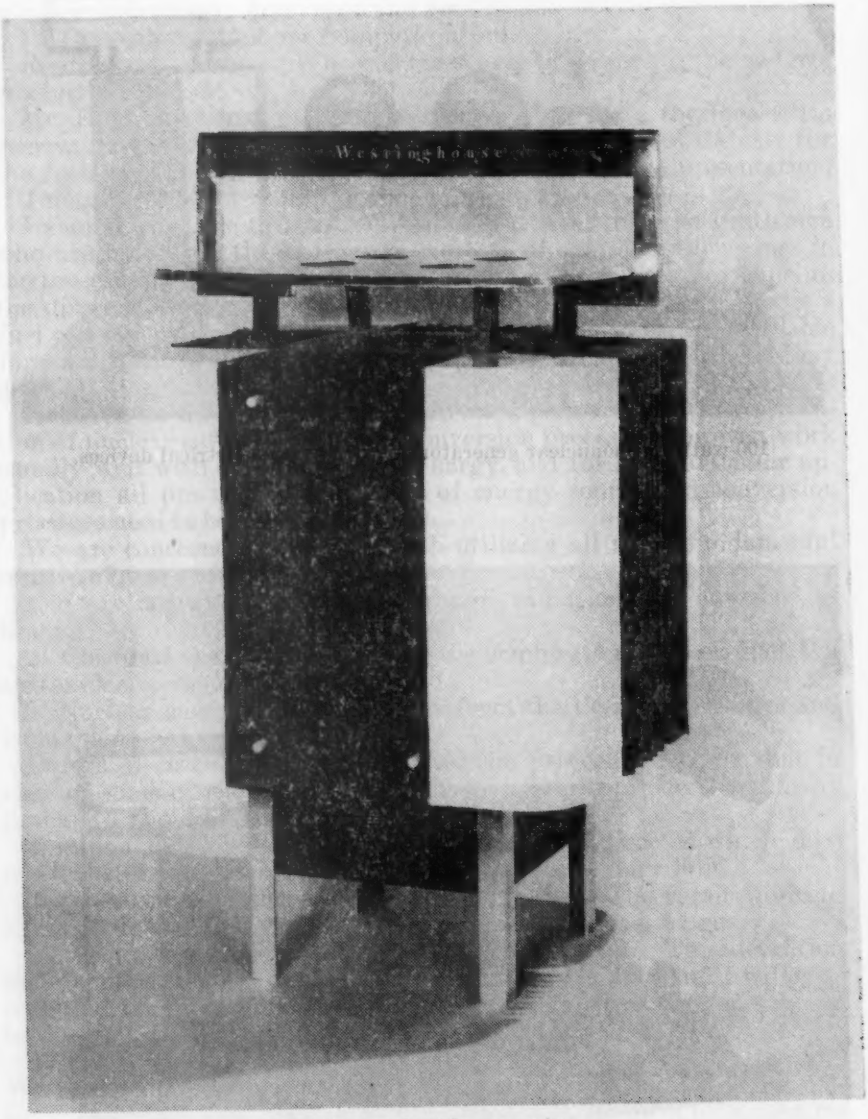
Magnetohydrodynamics, for example, though it is doubtful as a direct conversion process.



100-watt thermonuclear generator with operating electrical devices.



Grinding wheel operated by thermoelectric generator.



Thermoelectric generator.

It depends on the particular system because in some cases the plasma is a secondary working medium.

In any event, magnetohydrodynamics will have to wait for the development of high temperature materials before it is feasible. Pyroelectricity and various other possibilities exist, none of which look promising except for very special application.

So it is the first four which are receiving the most attention. The most highly developed today are the solar cells utilizing energy in the form of radiation. In the most familiar form a very perfect silicon crystal is treated with traces of impurities that introduce extra positive and negative charges on the two sides of a thin wafer (fig., p. 312).

These extra charges remain on their own side of the junction that separates the two sides until light energy strikes the crystal.

This radiant energy further disturbs the electrical balance of charges and starts them moving toward the surfaces and the junction. This current flow then continues as long as light strikes the crystal.

Mr. EGLI. The physics of this process is reasonably well understood, and the theory tells us that as much as 25 percent of the incident radiation can be converted to electricity, under special conditions possibly as high as 40 percent.

The best now reported from the laboratory is 14 percent and substantial production quantities can be obtained that give a 10-percent conversion efficiency. The cells are reliable and operate indefinitely with no loss in efficiency.

These represent the best available means of providing small amounts of power for satellite communications. The cells last so long, in fact, that a time switch must be incorporated to shut off transmission after the desired interval. Solar cells are limited to small power supplies, however, because of their weight. Bare cells produce only 3 watts per pound.

In a satellite, they must be distributed over the surface of a free tumbling sphere or a mechanism must be provided to keep them pointed at the sun.

A third possibility is to use the solar cells to charge batteries which in turn provide current during dark periods, but all of these schemes add weight so that the complete power package delivers less than 1 watt per pound.

Stated in another manner, the power supply weighs more than 1,000 pounds per kilowatt, and this is far too heavy to boost into space.

As large power supplies for ground stations, solar cells have a problem because of the size and cost. A 10-kilowatt generator suitable for a modern home power supply, for example, would occupy 100 square yards and cost $2 million.

Most of the research effort is directed toward improved efficiency by growing more perfect crystals and by experimenting with different materials such as gallium arsenide and cadmium sulfide.

Some increased efficiency can be expected, but probably by no more than a factor or two.

For space applications it would appear more profitable to attack the weight problem directly because the active region of the crystal is only a few millionths of an inch thick, a tiny fraction of the total thickness of the present wafers.

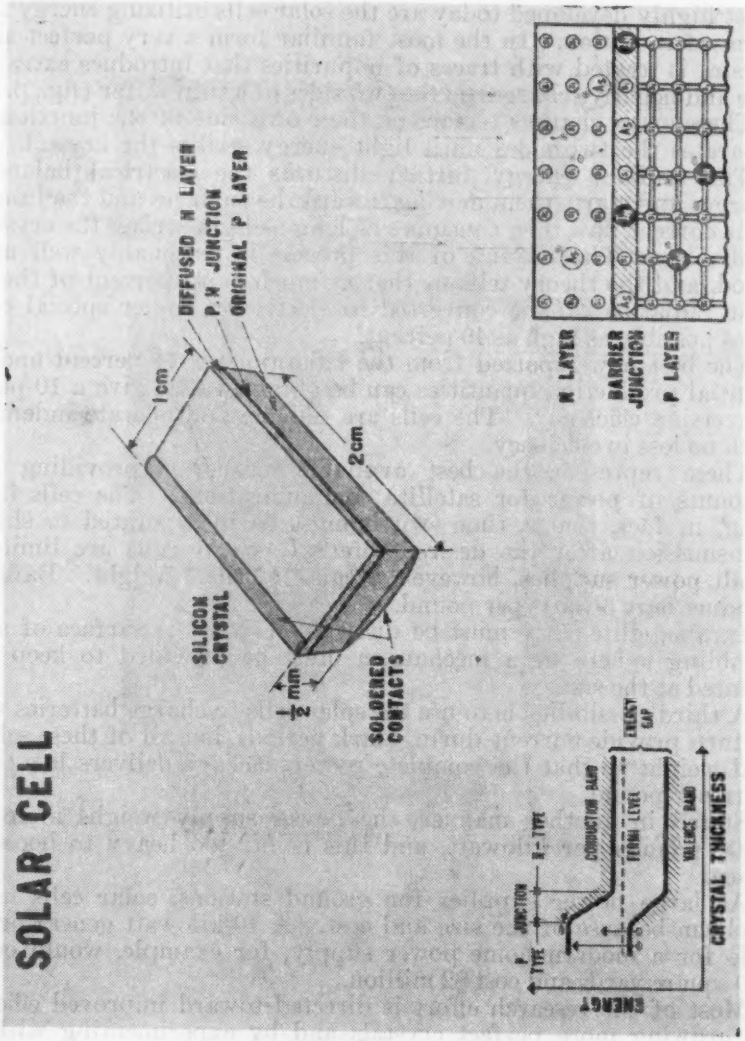

SOLAR CELL
DIFFUSED N LAYER
P-N JUNCTION
ORIGINAL P LAYER
1cm
2cm
SILICON CRYSTAL
1/2 mm
SOLDERED CONTACTS
N LAYER
BARRIER JUNCTION
P LAYER
ENERGY
P TYPE
JUNCTION
N - TYPE
CONDUCTION BAND
FERMI LEVEL
ENERGY GAP
VALENCE BAND
CRYSTAL THICKNESS

FIGURE 1

There appears to be much room for improvement by using crystals in the form of thin films, and substantial improvements must be made for solar cells to remain competitive as other devices are developed. The best projection using existing solar cell techniques predicts 120 pounds per kilowatt, which is still much too heavy.

Another method for the utilization of solar energy is by collecting it and absorbing the heat content.

For example, a mirror could be employed to collect and focus the sun's energy into a metal cylinder, where the concentrated heat could be used to operate thermocouples or thermionic diodes (fig. 2, p. 314).

In this instance heat would flow from the hot interior of the cylinder through the energy converson device creating electricity, and the waste heat would be radiated into space, or in case of a ground installation would be discharged into a body of water.

Mr. EGLI. The heat conversion process which is receiving most attention at the present time is thermoelectricity. This is basically a very simple phenomenon (fig. 3, p. 315).

If a temperature difference is maintained across any bar of a material which conducts electricity, the electrons at the hot end of the bar move about more vigorously and tend to drift toward the cold end.

It is entirely proper to say that heat pushes electricity through the bar. In some materials it is negative charges that move and in other materials the positive charges move.

These opposite effects can be added together when the two types of materials are properly selected and joined to form a thermocouple.

This is a very old process. It was discovered in 1822 and efficiencies as high as 3 percent could be obtained in 1850. This was a higher efficiency than was obtained by the steam engines of that day.

The scientists involved took a wrong turn, however, and no further progress was made except that thermocouples have been widely used to measure temperature.

Research in physics has recently taught us that to produce substantial amounts of power by the thermoelectric process, metals are the wrong materials. Metals have so many free electrons that increasing the temperature simply causes them to become more crowded with little chance to drift.

It is now recognized that semiconductors such as lead telluride, which have much smaller numbers of free electrons but with more freedom to drift, can produce substantially larger voltages and currents.

The physics of thermoelectricity is now reasonably well understood, and the theory indicates that efficiencies as high as 35 to 40 percent might be achieved.

To accomplish the 35 to 40 percent efficiency for thermoelectricity the materials must have precisely the optimum set of properties in the face of a number of conflicting requirements. As previously stated, the number of free electrons must be small in order to generate a large voltage. But the number of electrons must be large enough to avoid losses from internal resistance which generates heat that flows in the wrong direction. And finally, the material must have a low thermal conductivity so that heat cannot flow through the material without doing some electrical work. The optimum compromise that must be reached is shown in the lower left diagram of figure 3.

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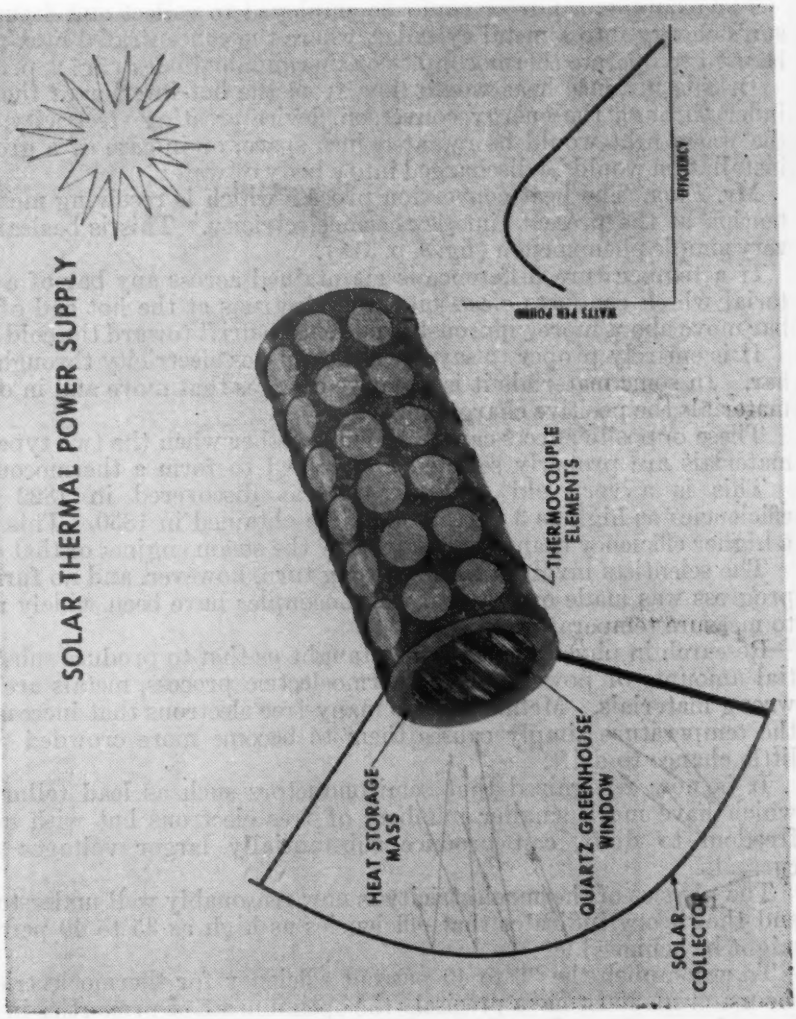

SOLAR THERMAL POWER SUPPLY
WATTS PER POUND
EFFICIENCY
THERMOCOUPLE ELEMENTS
HEAT STORAGE MASS
QUARTZ GREENHOUSE WINDOW
SOLAR COLLECTOR

FIGURE 2

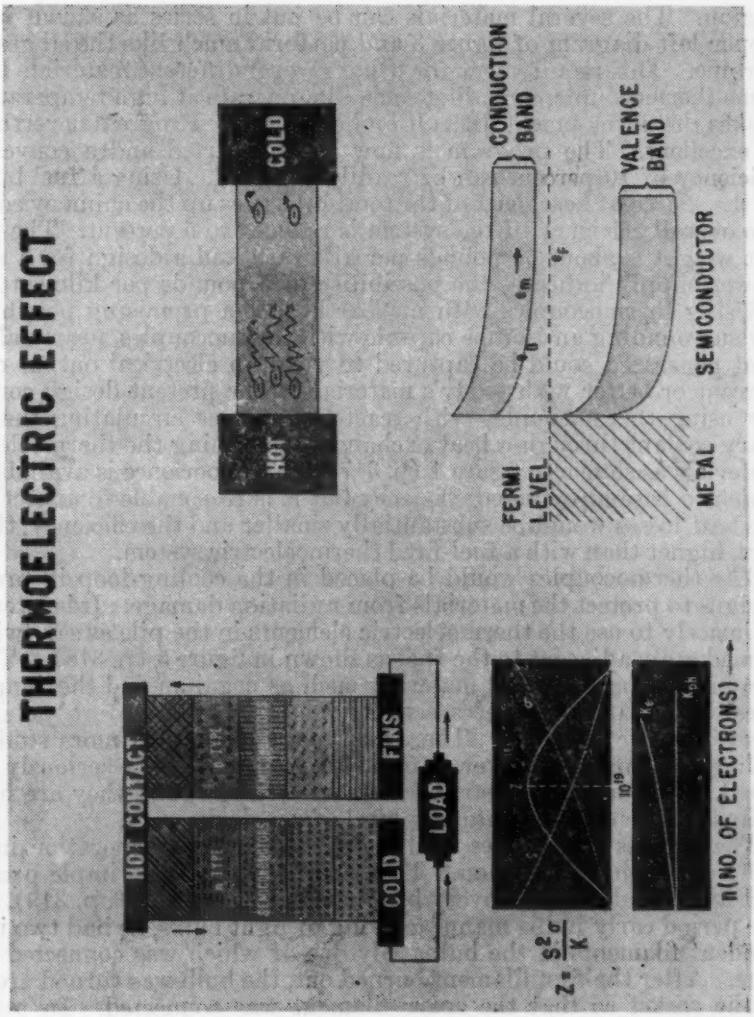

THERMOELECTRIC EFFECT
HOT CONTACT
COLD
FINS
LOAD
COLD
HOT
CONDUCTION BAND
VALENCE BAND
FERMI LEVEL
METAL
SEMICONDUCTOR
$Z = \frac{S^2\sigma}{K}$
n(NO. OF ELECTRONS)

FIGURE 3

To complicate the situation further, each of these important properties of the material changes with temperature so that for any given material the best compromise of properties will exist for only a small temperature range. A thermocouple is a simple heat pump and like all other heat engines, the efficiency increases with larger temperature differences. To operate over a wide temperature range requires, however, a number of materials, each efficient in its small temperature region. The several materials can be put in series as shown in the upper left diagram of figure 3 and perform much like the stages of a turbine. This requirement for using several different materials in the same thermocouple, including some that operate at high temperatures, makes the development difficult and expensive. Progress nevertheless is excellent. The program is now barely started and a conversion efficiency of 10 percent can be readily obtained. Using a fuel burner as the source of heat, half of the total heat goes up the chimney so that the overall efficiency of the system is reduced to 5 percent. The present weight is about 65 pounds per kilowatt, and a design for a space power supply indicates the possibility of 5 pounds per kilowatt.

Using thermocouples with nuclear heat is a promising possibility. By surrounding an isotope capsule with thermocouples, nearly all the heat generated could be captured to yield an electrical output of 10 percent or better with today's materials. The present design concept for using thermocouples with reactors involves circulating the primary coolant through a heat exchanger containing the thermoelectric materials as shown in figure 4 (p. 317). No experience is available on which to base a predicted efficiency but it is reasonable to expect that the heat losses would be substantially smaller and the efficiency, therefore, higher than with a fuel-fired thermoelectric system.

The thermocouples would be placed in the cooling loop in present designs to protect the materials from radiation damage. It is intended ultimately to use the thermoelectric elements in the pile as sandwiches immediately adjacent to the fuel as shown in figure 5 (p. 318). There are, in fact, possible fuel materials such as uranium and thorium sulfides which are themselves semiconductors and therefore potential thermoelectric elements. There must be a great deal more study of radiation damage, however, before such a system can be seriously proposed. Although very early results are encouraging, they are by no means sufficient to indicate any kind of conclusion.

The process which does lend itself readily to incorporation in the pile is thermionic emission. This, too, is basically a simple process, and it is based on a discovery by Thomas Edison (fig. 6, p. 319). At one period early in his manufacturing of light bulbs, he had two independent filaments in the bulb, only one of which was connected at a time. After the first filament burned out, the bulb was turned around in the socket so that the spare filament was connected. In testing these bulbs, Edison discovered that he could draw a small current from the filament that was not connected. Some electricity was moving through the vacuum in the bulb from the hot filament to the cold. This is the basis on which all radio tubes operate, and the same process now promises to become an efficient energy converter.

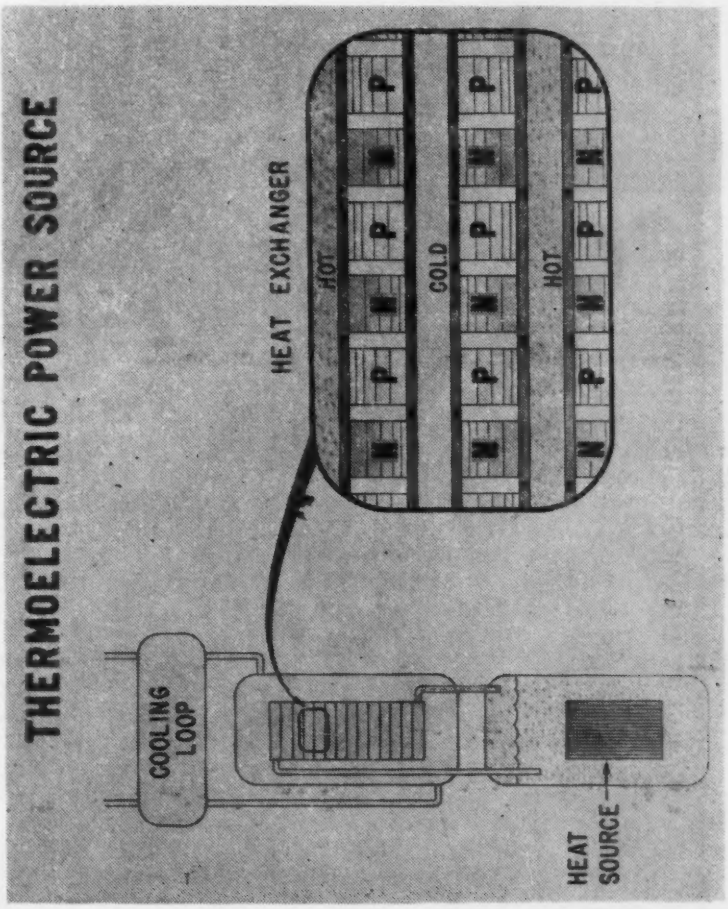

THERMOELECTRIC POWER SOURCE
HEAT EXCHANGER
HOT
COLD
HOT
N
P
COOLING LOOP
HEAT SOURCE

FIGURE 4

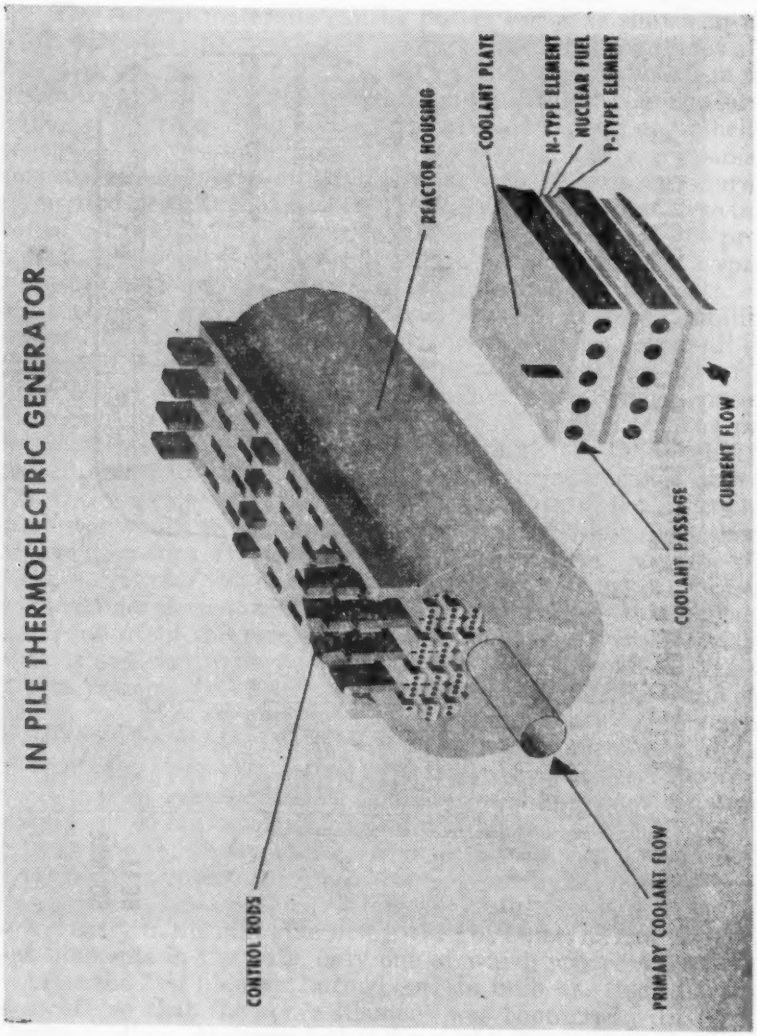

IN PILE THERMOELECTRIC GENERATOR
REACTOR HOUSING
COOLANT PLATE
N-TYPE ELEMENT
NUCLEAR FUEL
P-TYPE ELEMENT
CONTROL RODS
COOLANT PASSAGE
CURRENT FLOW
PRIMARY COOLANT FLOW

FIGURE 5

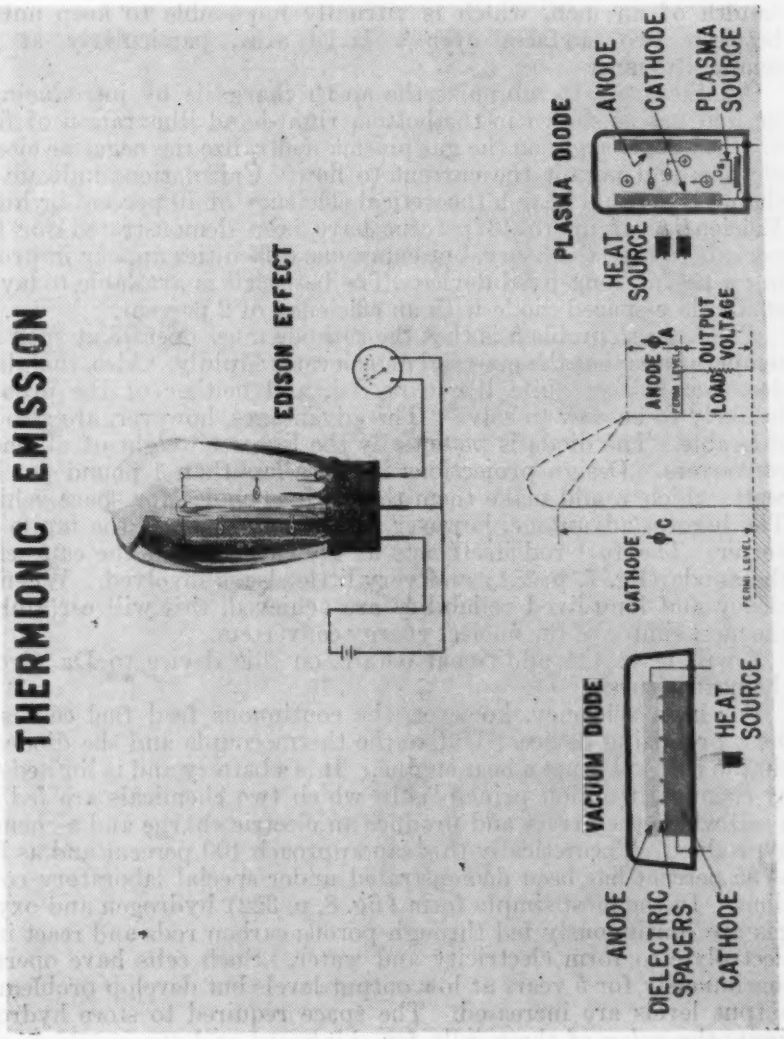

THERMIONIC EMISSION
EDISON EFFECT
VACUUM DIODE
ANODE
DIELECTRIC SPACERS
CATHODE
HEAT SOURCE
CATHODE
FERMI LEVEL
PLASMA DIODE
ANODE
CATHODE
PLASMA SOURCE
HEAT SOURCE
ANODE
FERMI LEVEL
LOAD
OUTPUT VOLTAGE

FIGURE 6

To generate substantial amounts of power by the thermionic process, it is necessary for large numbers of electrons to flow from the hot cathode to the cold anode. Because the electrons are all negatively charged, they repel each other and build up a charge that limits further flow. This problem can be minimized by putting the cathode and anode very close together, as shown in the bottom left diagram of figure 6. The spacing required, however, is less than a ten-thousandth of an inch, which is vitrually impossible to keep uniform between two surfaces over a large area, particularly at high temperatures.

Another way to minimize the space charge is by introducing an ionized gas as shown in the bottom right-hand illustration of figure 6. Positive charges on the gas plasma neutralize the negative electron charges and permit the current to flow. Calculations indicate that the process could have a theoretical efficiency of 40 percent or higher. Efficiencies of up to 13 percent have been demonstrated for brief periods in the laboratory, but enormous difficulties appear in producing a useful, long-lived device. The best that is available today is a small close-spaced diode with an efficiency of 2 percent.

The biggest problem is that the cathode must operate at such high temperatures that the material deteriorates rapidly. Also, the plasma does not behave quite like expected, and neither of the problems is likely to be easy to solve. The advantages, however, are also considerable. The diode is potentially the lightest weight of all energy converters. Design projections indicate less than 1 pound per kilowatt, which would make them the obvious choice for space vehicles. The biggest advantage, however, is the simplicity of the inpile converter. The fuel rod itself acts as the cathode and the can acts as the anode (fig. 7, p. 321) and very little else is involved. When efficiency and long-lived reliability are achieved, this will certainly be the most simple of the nuclear energy converters.

I will leave the additional details on this device to Dr. Grover, the next witness.

For high efficiency, however, the continuous feed fuel cell is the most promising device. Unlike the thermocouple and the diode, the simple fuel cell is not a heat engine. It is a battery and is limited only by chemical reaction principles in which two chemicals are fed into a cell where they react and produce an electric charge and a chemical byproduct. Theoretically this can approach 100 percent and as high as 95 percent has been demonstrated under special laboratory conditions. In the most simple form (fig. 8, p. 322) hydrogen and oxygen gas are continuously fed through porous carbon rods and react in an electrolyte to form electricity and water. Such cells have operated continuously for 5 years at low output levels but develop problems as output levels are increased. The space required to store hydrogen limits the value of these cells for shipboard and space vehicle uses, but many other applications look promising.

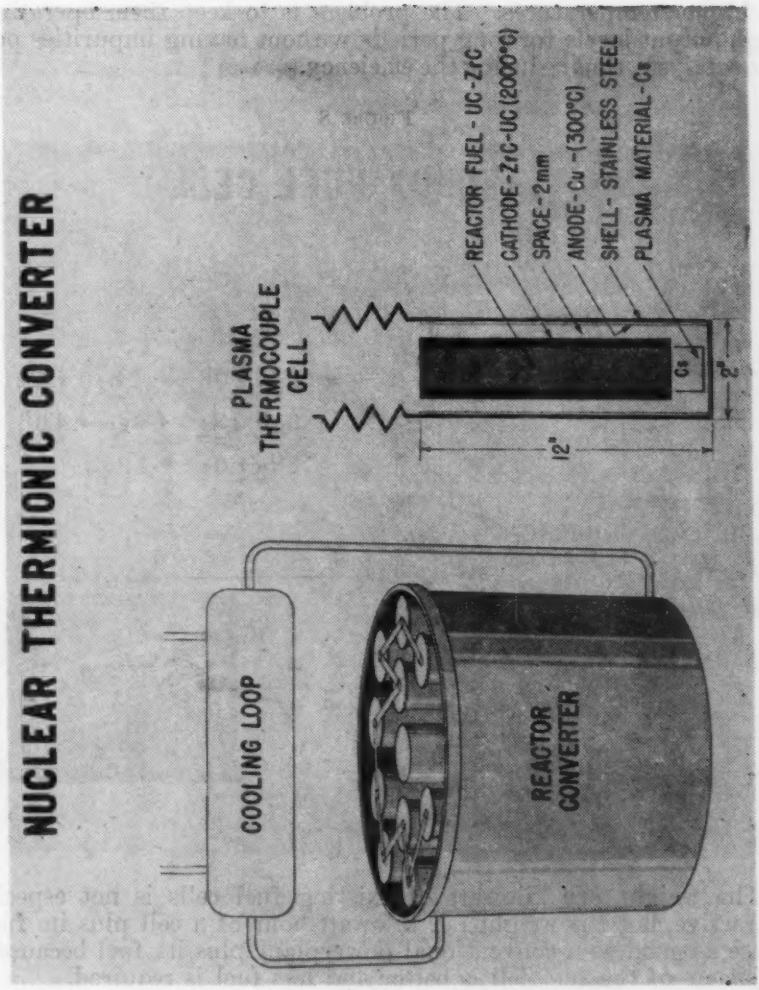
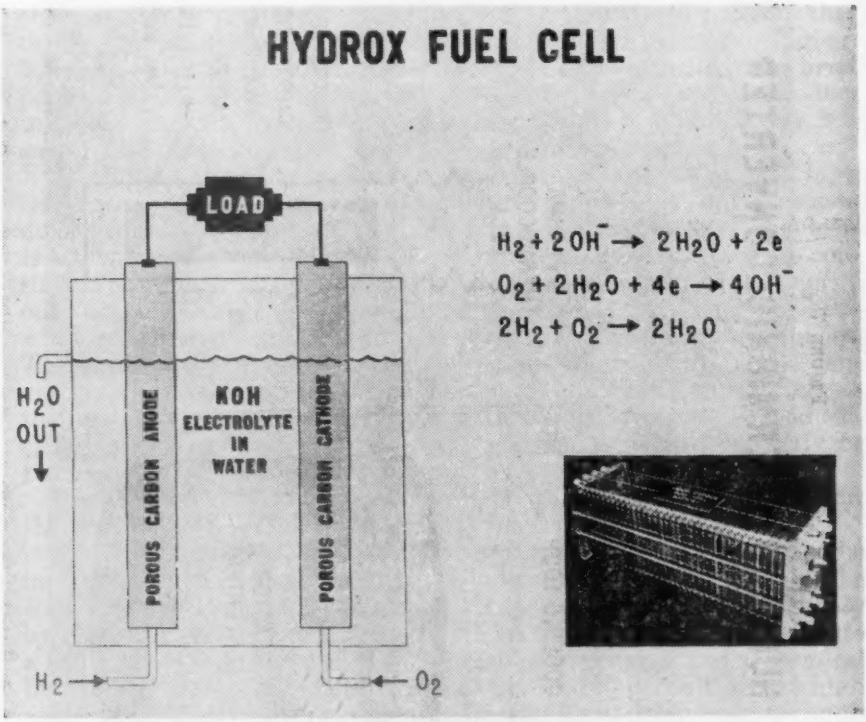

NUCLEAR THERMIONIC CONVERTER
COOLING LOOP
REACTOR CONVERTER
PLASMA THERMOCOUPLE CELL
REACTOR FUEL - UC-ZrC
CATHODE - ZrC-UC (2000°C)
SPACE - 2mm
ANODE - Cu ~(300°C)
SHELL - STAINLESS STEEL
PLASMA MATERIAL - Cs
Cs
2"
12"

FIGURE 7

Dozens of variations of fuel cells exist utilizing a variety of chemicals. The most exciting possibility is a fuel cell which would use conventional petroleum products and air as the fuels; and several companies have such cells in early stages of development. The general scheme is to use catalysts in the electrodes which would separate hydrogen from the hydrocarbons and use the oxygen from the air as the other fuel. Such cells have been demonstrated with efficiencies of better than 35 percent at low temperatures, and up to 75 percent at higher temperatures. The problem is to keep them operating at high output levels for long periods without having impurities poison the catalysts, thus reducing the efficiency.

FIGURE 8



The weight per kilowatt of existing fuel cells is not especially attractive, but the weight per kilowatt-hour of a cell plus its fuel is twice as good as a conventional powerplant plus its fuel because the efficiency of the fuel cell is better and less fuel is required.

Success in these devices could obviously have a large impact on the whole problem of power supplies. An efficiency of 60 to 80 percent is a very reasonable goal; and this could result in doubling the life of our fossil fuel reserves.

Fuel cells can also be operated as closed cycles activated by nuclear heat. The chemicals formed by the battery action could be decomposed by heat to re-form the original chemicals used in the fuel cell and recirculated indefinitely (fig. 9, p. 323). This is a heat engine

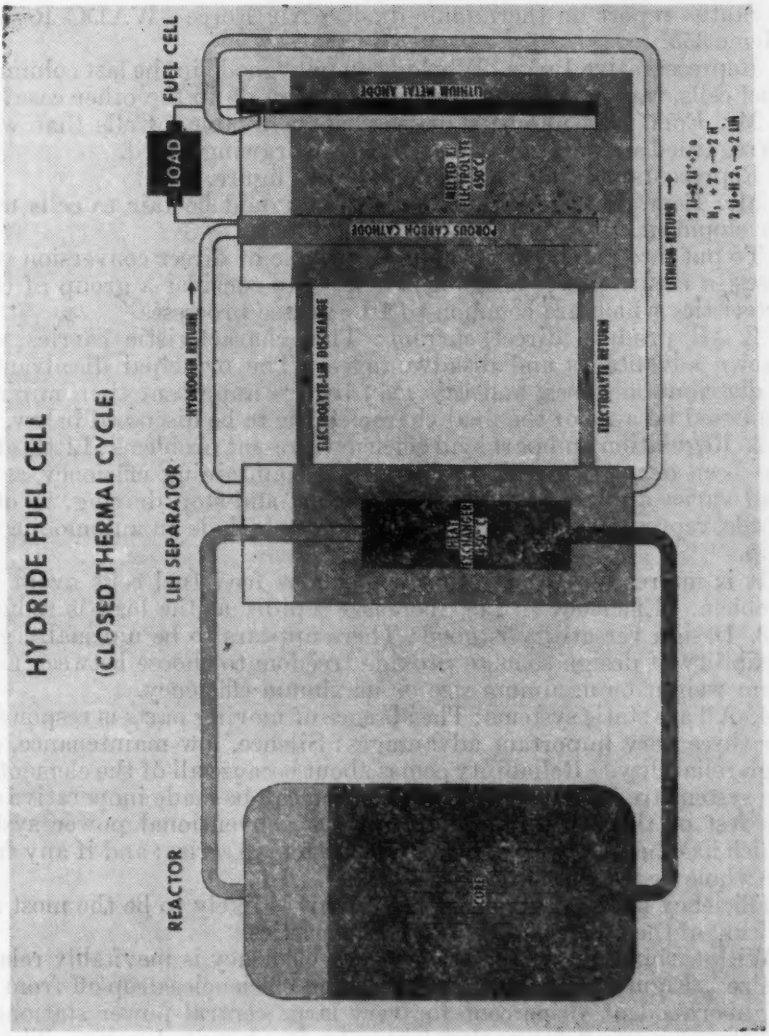

HYDRIDE FUEL CELL
(CLOSED THERMAL CYCLE)
FUEL CELL
LOAD
LiH SEPARATOR
REACTOR
CORE
HYDROGEN RETURN
ELECTROLYTE-Li DISCHARGE
ELECTROLYTE RETURN
LITHIUM RETURN

FIGURE 9

again, however, limited to less than 35 percent efficiency, and the advantages over the more simple heat engines are not obvious.

For further details on the various processes I should like to introduce for reference status reports prepared by the three military services:

Status report on thermoelectricity—NRL Memorandum Report 1037.

Status report on fuel cells, Army—ARO Report No. 1.

Status report on thermionic diodes, Air Force—WADC Research Memo 355.

Representative PRICE. Why do you put "good" in the last column for fuel cells, rather than put the figure as you do in the other case?

Mr. EGLI. The numbers just are not available. Cells that would be regarded as operational are still on the drawing board.

Representative PRICE. You don't have a figure, then?

Mr. EGLI. That is right. None that would be fair to cells under development.

To put into perspective the probable role of direct conversion processes in future power usage it is helpful to consider a group of characteristics which are common to all of these processes.

1. All produce direct current: This characteristic carries well-known advantages and disadvantages. The principal disadvantage is distribution losses, and this may be less important than might be supposed because of the final characteristic to be discussed below.

2. Regulation and part load efficiency present problems: Little effort has been devoted to this problem, but maintaining efficiency as the load varies appears to be difficult. Start-and-stop driving, in other words, represents a problem whether the vehicle is an automobile or a ship.

It is interesting to note that continuous feed fuel cells avoid this problem. Efficiency in fact increases rapidly as the load is reduced.

3. Design versatility is good: There appears to be unusually good flexibility of design so as to provide freedom to choose between minimum weight or minimum size or maximum efficiency.

4. All are static systems: The absence of moving parts is responsible for three very important advantages: Silence, low maintenance, and high reliability. Reliability comes about because all of the elements in the system are in parallel. Part of them can be made inoperative and the rest of them still function unlike a conventional power system which has the various elements in the system in series; and if any fails, the whole system is out of operation.

Efficiency is independent of size. This is likely to be the most significant of the direct conversion characteristics.

With turbines or combustion engines, efficiency is inevitably related to size. Figure 10 (p. 325) indicates that efficiencies drop off from the neighborhood of 40 percent for very large central power stations to less than 5 percent for powerplants of a few hundred watts. The entire area above and to the left of the curve for rotating machinery on figure 10 must be solved by direct conversion. A small efficient turbine plant using nuclear heat cannot be built. Conversely, a large direct conversion plant for central distribution would have no advantake over a small direct conversion plant located where the power is to be used. Central distribution thus offers no advantages with direct

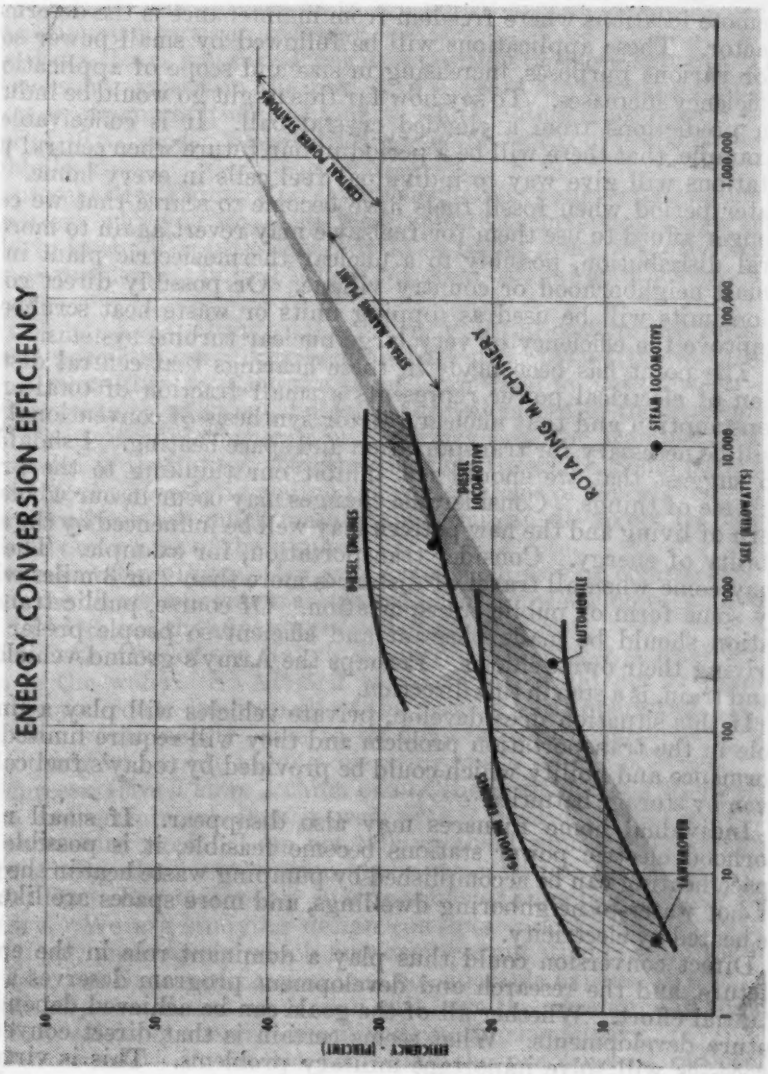

ENERGY CONVERSION EFFICIENCY
ROTATING MACHINERY
STEAM ALREADY PLANT
CENTRAL POWER STATIONS
STEAM LOCOMOTIVE
HYDROPOWER
DIESEL
DIESEL ENGINES
AUTOMOBILE
GASOLINE ENGINES
AIRPLANE
EFFICIENCY - PERCENT
SIZE (KILOWATTS)
60
50
40
30
20
10
1,000,000
100,000
10,000
1000
100
10
1

FIGURE 10

conversion so distribution losses of the d.c. power is not a serious problem.

The total impact of direct conversion remains to be determined by future developments. It is not expected that any one direct conversion process will be a panacea for all power problems. Each will find applications based on its peculiar virtues. The first uses will be in applications where silence is more important than efficiency and in remote locations where freedom from maintenance is the determining factor. These applications will be followed by small power sources for various purposes, increasing in size and scope of applications as efficiency increases. To say how far this might go would be indulging in predictions from a clouded crystal ball. It is conceivable, for example, that there will be a period in our future when central power stations will give way to individual fuel cells in every home. At a later period when fossil fuels have become so scarce that we can no longer afford to use them for fuels, we may revert again to more central distribution, possibly to a nuclear thermoelectric plant in each small neighborhood or country village. Or possibly direct conversion units will be used as topping units or waste heat scrubbers to improve the efficiency of very large nuclear turbine systems.

The point has been made in these hearings that central distribution of electrical power represents a small fraction of total energy consumption and that nuclear reactor synthesis of conventional fuels will be necessary for transportation and space heating. I should like to suggest that we should not inhibit our thinking to the present scheme of things. Considerable changes may occur in our whole pattern of living and the new pattern may well be influenced by the availability of energy. Consider transportation, for example. The time may come when all travel of distances more than 2 or 3 miles will be by some form of public transportation. Of course, public transportation should be made pleasant and efficient so people prefer it to driving their own vehicles. Perhaps the Army's ground vehicle, the land train, is a step in this direction.

If this situation does develop, private vehicles will play a limited role in the transportation problem and they will require limited performance and ability which could be provided by today's fuel cells or even by storage batteries.

Individual home furnaces may also disappear. If small neighborhood electric power stations become feasible, it is possible that space heating can be accomplished by pumping waste heat in the form of hot water to neighboring dwellings, and more spaces are likely to be heated by electricity.

Direct conversion could thus play a dominant role in the energy picture, and the research and development program deserves a substantial effort. Whether all of the goals can be achieved depends on future developments. What seems certain is that direct conversion processes will solve important military problems. This is virtually assured, and it seems reasonable to expect that progress made in connection with these developments and applications will lead to increasing civilian uses.

Representative PRICE. That is a very fine and interesting statement, and the presentation using the excellent charts has been very well done by you and your assistants.

Mr. EGLI. Thank you, sir.

Representative PRICE. Do you have any questions, Mr. Hosmer?

Representative HOSMER. No. I would like to say that Mr. Egli's presentation of a very difficult subject has been the clearest one we have had in a long time and he deserves to be congratulated.

Mr. EGLI. Thank you, sir.

Representative PRICE. Mr. Egli, it is a difficult subject and therefore it is very difficult for us to get our teeth into your overall presentation. Can you make any guesses as to the potential cost of energy from these various conversion processes?

Mr. EGLI. I am not sure I am in a position to quote figures. The efficiencies we expect in the next 3 to 5 years are perhaps half the efficiencies from rotating machinery. The maintenance should be a factor of 10 reduced. The overall cost should work out to about a comparable level.

Representative PRICE. You say at one place in your statement one of the reasons why you would go into the direct conversion usage, particularly in small applications, would be to get away from the maintenance problems. Is that a very big item as against other types of energy?

Mr. EGLI. Yes. Maintenance is a substantial cost in any form of present power sources, approaching half probably of the total cost. Here I would like to make the point again that we not only have no moving parts to wear out, but all of the elements in the system are lined up in parallel so that if in a military case, for example, half of the system is shot away, the other half still works. Some parts can fail without seriously impeding the performance of the whole system. There really is not much maintenance you can do except to replace modules or sections of the system.

Representative PRICE. Can you draw some sort of comparison between the direct conversion as against a small nuclear powerplant like the SNAP 8, and the advantages and disadvantages?

Mr. EGLI. One of the Navy's interests is for power sources buried deep in the water. SNAP 8, I would think, would not be feasible, because you could not keep all of the moving parts operating. A job like this would nearly have to be done by direct conversion. The same thing will apply to long-operating space vehicle missions.

Representative PRICE. I think one of the presentations yesterday—I am not certain whether it was SNAP 8—did picture a reactor for submarine work. I am not talking about submarine propulsion. I am talking about exploration of the ocean bottom and so on.

Mr. EGLI. For reasonably short periods of time it is entirely satisfactory. We are studying design concepts which simplify rotating machinery enormously which may well permit it to operate for long periods, but the simplification is at considerable expense to efficiency. It is likely that direct conversion will have the advantage in the case of better efficiency than the simplified rotating machinery.

Representative PRICE. Do you think the problems of radioactive waste or radioactivity of isotope heat sources would constitute serious drawbacks to small-scale applications such as you described in the final page of your paper, where the direct conversion process is based on nuclear energy sources.

Mr. EGLI. I would prefer qualified people to consider that problem. About the smallest system I proposed was a reactor for the Army's

land vehicle. Presumably there are design concepts that look favorable for that size operation.

Representative PRICE. Mr. Hosmer.

Representative HOSMER. Mr. Egli, a question has occurred to me in relation to the extent of the effort going on in this field by Government and by private industry. We have been thinking a lot in terms of programs. A program for space and a program for this, that, and the other thing. Who is interested in this and who is doing something about it, and to what extent? Could you review that for us? I think you are actually the one man who has to keep his eyes open across this field, are you not?

Mr. EGLI. We certainly try to keep well informed; yes. Communication in this field and the cooperation between the services and AEC is excellent. The most aggressive program for improvement of the conversion processes is with the armed services. If you want a dollar figure, the entire direct conversion research and development effort is somewhat under $10 million. That excludes some systems work and parts of the SNAP program. But the work that is directly related to improving the conversion processes is of that order. The services tend to emphasize the process that best fits their requirements, so that the Navy's primary interest has been and is in thermoelectricity. The Army has long been the battery source, and has the largest program in fuel cells. The Air Force, because of the attractive light-weight feature of thermionic diodes has emphasized that area. Each of us has small programs in each of the processes in order to keep well informed and to consider them for their applications. Is that a sufficient answer to give you an idea?

Representative HOSMER. Can you give us an idea about the industry effort? You can leave out fuel cells because we know that it is large.

Mr. EGLI. On fuel cells industry is doing an enormous amount of work, many times as much as the military is supporting. For thermoelectricity and thermionic diodes, I would say the industry level is just about comparable to the Government effort. Most of our contractors are doing as much or more on their own as the work we are supporting.

Representative HOSMER. Does the tunnel diode have application here?

Mr. EGLI. It is a possible, although not probably the best way to convert DC to AC. It could be used as a part of a solid state converter.

Representative HOSMER. Thank you.

Representative PRICE. I would like to stay on the question that Mr. Hosmer asked you but to put it a little different way. Do we have enough research effort going into the direct conversion program at the present time, in your opinion?

Mr. EGLI. I would say that we have operated the program somewhat conservatively. There were very serious barriers that had to be crossed. The question of whether any material could be an efficient thermoelectric material at high temperature, for example, was a serious problem, and the effect of radiation damage needed immediate attention. So the program was small and directed specifically at those critical questions. Within the past year some of those or most of those barriers have been crossed. We are now in the process of about doubling the size of the program. It is still reasonably modest, compared to many other programs, still directed to some of the specific questions

that mean success or failure. Assuming that our present request to roughly double the size of the program is supported, I would say it is at a reasonable level.

Representative PRICE. Just to help us in our thinking on it, and this is not always a true barometer of the size of the program, moneywise, how big a program is it?

Mr. EGLI. On the order of $10 million for direct improvement of the conversion processes. That is excluding SNAP system developments and so forth. It is fairly modest.

Representative PRICE. You say it is fairly modest. Does it meet the requirements as set up by those in charge of the program, or have they at times thought it should be at a higher level and have not succeeded in getting the support that they desire?

Mr. EGLI. It is always difficult to launch a new program under all of the budget requirements. I would say that this one has received sympathetic hearings and good support.

Representative PRICE. How do you operate on the basis of requirements? Where do your requirements come from, or where do the suggestions for exploration originate?

Mr. EGLI. In the early stages of the program, so long as it is primarily a research effort, there does not need to be a specific device goal. We have operated primarily on the basis of the sort of chart I showed, that there are many requirements for small power sources which can be solved best in this way. Obviously we are interested in silent ship engines. However, the size of the program that is now in progress does not need a particular size ship engine as its immediate goal.

Representative PRICE. Thank you very much for a very interesting presentation.

Mr. EGLI. Thank you, sir.

Representative PRICE. The next witness this morning is Dr. George M. Grover, of the Los Alamos Scientific Laboratory.

Your presentation this morning, Dr. Grover, will be on thermionic cell development.

STATEMENT OF GEORGE M. GROVER,[1] LOS ALAMOS SCIENTIFIC LABORATORY

Dr. GROVER. Within the last year there has been a great deal of attention given to a method of direct conversion of heat to electricity which is called generally thermionic conversion.

Actually, work along this line has been going on for several years in university, industrial, and national laboratories. This work was

[1] Group leader of the advanced design group of the Rover project at Los Alamos Scientific Laboratory. This group is responsible for much of the Laboratory research on, and evaluation of, new nuclear propulsion concepts and of the direct conversion effort. During World War II, Dr. Grover worked on meteorological and radar projects of the Weather Bureau and the OSRD in the Amazon Valley and in the Pacific theater of operations. In 1945, he was engaged in the proximity fuze development at the Johns Hopkins Applied Physics Laboratory. After joining the Los Alamos Laboratory in 1950 he was responsible for design and testing work on the atomic artillery projectile. In 1952, he participated in the design and construction of the first thermonuclear device, and later coordinated the work on the design and stockpiling of the first thermonuclear weapons. In 1955, Dr. Grover organized and directed the engineering design group working on the nuclear rocket program, and in 1958 organized and has since directed the effort of the advanced design group which developed the plasma thermocouple method of direct conversion of nuclear energy to electricity. He is a member of Sigma Xi and the American Physical Society.

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started quite independently and we now have several different approaches to the problem.

At Los Alamos, we have been primarily concerned with the cesium plasma diode which has been called a plasma thermocouple. The work was initiated as part of the advanced nuclear propulsion studies under the Rover program.

About 10 months ago, as you may recall, we demonstrated the conversion of fission energy into electricity by means of a small plasma thermocouple cell placed in the Omega West reactor at Los Alamos. Since that time we have continued our experiments and calculations.

On the desk is a cutaway of the first cell put into the Omega West reactor. The output of this cell was of the order of 30 watts, at voltages like 4 volts open circuit voltage, and short circuit currents of the order of 40 amperes from this small cell.

To understand how the plasma thermocouple works, we must first talk about plasmas and how they can be used to convert heat into electrical energy.

The Sherwood project has introduced the concept of a plasma as an electrically neutral gas consisting of ions and electrons. In the Sherwood applications they require temperatures of millions of degrees to obtain the fusion reaction.

It is possible, however, to generate plasmas at much lower temperatures. In fact, several gaseous elements will form plasmas at temperatures around 2,000° C. Of all the available materials, cesium vapor is most easily ionized to form a plasma. Of course, cesium will not fuse and produce energy in the Sherwood fashion. But these low-temperature plasmas have interesting characteristics and they can be contained in ordinary metal walls instead of magnetic bottles.

A characteristic of cesium plasmas is that the electrons are moving around in the plasma many times faster than the cesium ions, therefore the electrons will hit the container walls much more often than will the ions.

If the walls are cool, the electrons will be absorbed. If we connect a wire to this container, we can lead the electrons through the wire, through a load such as a light bulb or an electric motor, and then we must return these electrons back into the plasma.

In principle, then, to generate electricity, one needs only a source of heat to generate a plasma, a cool collector to pick up the electrons from the plasma and an external connection to return these electrons back to the plasma.

The properties of plasmas are being widely investigated. A great amount of research in this field was done about 20 years ago. Interest has been restimulated by the Sherwood work and is now extending to the relatively low-temperature plasma of interest in direct conversion.

Any source of heat at a relatively high temperature could be used to generate the plasma. Concentration of the sun's rays by mirrors or lenses can be used. Several industrial laboratories are working on the use of plasma cells for solar energy conversion. Fossil fuels can be burned to furnish energy at these temperatures, however, one must design against excessive flame corrosion and loss of most of the energy up the smokestack.

Nuclear fission, on the other hand, can provide energy at a very high temperature. A compound of uranium which has many desirable

properties for the plasma cell application is uranium carbide. This material is refractory, with a melting point well above 2,000° C. and it is a fairly good conductor of heat and electricity. It can be in contact with the cesium vapor to furnish heat to form the plasma and hot uranium carbide also emits a copious quantity of electrons to the plasma. It is probably the best electron emitter that we know in science today. This property allows its use as the return electrode into the plasma, in the thermionic cell.

The properties of uranium carbide and mixtures of other carbides with uranium carbide are interesting but not completely known. Mixing carbides, specifically zirconium carbide with uranium carbide, improve many of the mechancial properties without seriously degrading the desirable properties of good conductivity and electron emission.

In all reactors good thermal conductivity is of great importance. By contrast, the usual uranium compound in reactors, uranium oxide, is a good thermal insulator. For this reason extensive research is underway on the carbides for use in many different commercial and other reactors as well as for its specialized use in the plasma cell.

In a practical reactor design we can imagine the emitter of the cell to consist of a pencil of uranium carbide heated by internal nuclear fission and surrounded by a cylindrical metal collector, with cesium vapor inside this cylinder between the uranium and the walls.

Only four basic parts are involved: A uranium carbide pencil, a container, cesium vapor, and a suitable electrical insulator.

In appearance, perhaps the plasma cell would resemble a flashlight battery. These cells could be stacked in a long rod with the cells connected electrically in series. A bundle of these rods could be immersed in a tank of water which would serve as both a neutron moderator and a coolant and we would have a direct conversion nuclear reactor, with the addition of a few more details such as control rods, shielding, and so forth.

The pencils of the uranium carbide in each cell would constitute a critical assembly in water if a sufficient number of rods were used.

The fissioning of the uranium would heat each pencil of uranium carbide to incandescence, and the cell would generate electrical power. There would be hundreds of such rods and thousands of cells in a large power reactor. But the basic building block, the cell, is amenable to the techniques of mass production. The simplicity of such a device is readily apparent.

Nevertheless, there are problems such as high temperature materials technology, possible radiation damage, maximizing the conversion efficiency of the fission heat to electricity, and many others, which require solutions.

This reactor concept is one type being studied at Los Alamos. The examination of specific reactor concepts is only a small part of the major effort on basic research to understand more completely the characteristics of thermionic cells and of the materials of which they are composed.

In much of this research it is possible to use electrically heated cells as a basic research tool for obtaining a complete understanding of low temperature plasmas and their interaction with material surfaces.

Mr. RAMEY. What is the name of your project at Los Alamos?

Dr. GROVER. The plasma thermocouple project. This is a splitoff from the Rover program, the advanced design work in the Rover program.

Representative PRICE. You do not have a code name for it?

Dr. GROVER. There is no code name as yet. The splitoff from the Rover project is rather recent. It is not complete as yet, of course.

Mr. RAMEY. The main advantage of this kind of device, if you have it in service, would be that you do not have any of the conversion machinery, the turbine and generator and so on, that you would have in a large central station powerplant.

Dr. GROVER. That is correct.

Mr. RAMEY. You have a continuous nuclear reaction like you would in any nuclear powerplant, but you do not have to have all of the collateral equipment to be putting out your electric current.

Dr. GROVER. Yes. An interesting experimental approach being used is to fabricate single plasma cells complete with their pencils of uranium carbide and insert these into the central high neutron flux region of a swimming pool or tank reactor.

The neutrons from the other fuel rods in the reactor cause fissions in the uranium in the cell and the heat produced generates the cesium plasma and the operating characteristics of the cell can be fully determined.

It will be possible to develop a multicell conversion reactor rod using reactors now in operation and these tests are now going on. This will greatly simplify the task of developing the first complete plasma cell integral-test reactor.

Assuming that the developmental program now underway will provide answers to the many technical questions of lifetime, efficiency, and so forth, what is the most suitable field for the first application of direct conversion nuclear reactors using thermionic cells? Certainly, this is in our space program.

For a man to exist for any length of time in space will require substantial quantities of electrical energy.

This power will be needed to carry on the observations and experiments for which he is there to perform, to communicate, and just to exist in the alien environment of space.

The overall needs of our space program begin with power supply requirements ranging from those small enough to utilize solar energy up to a demand for units of thousands of kilowatts output, enough to supply a small city just for one space vehicle.

If these power supplies can be made light enough, they will not only furnish auxiliary power, but will make possible the ion or plasma propulsion of the vehicle.

This method of propulsion of space vehicles is under intensive study in many laboratories and many methods have been proposed for accelerating charged particles to extremely high velocities and expelling them from the vehicle to produce thrust.

The specific impulse, or pound of thrust per pound of propellant expelled, can be very high, many thousands as compared to about 400 for the best chemical rockets and about 900 for nuclear heat transfer rockets.

These ion rockets are always low total thrust devices—they can never lift themselves off the earth's surface against gravity.

This is a practical limitation, I might add. The specific power that would be required in a power supply to lift a vehicle off the ground into a low-level orbit would be a value like 100,000 to 500,000 watts per pound of power supply.

However, if a vehicle having an ion motor were boosted into an orbit around the earth, then the low-thrust, high-specific-impulse rocket operating for several days or even several weeks, would provide the additional velocity for the vehicle to escape from the earth.

After a coast period of 2 months or so, the vehicle could be decelerated into an orbit around another planet.

This rather slow transfer time from one orbit to another is common to all rocket systems, chemical, nuclear heat transfer, and ion propulsion.

However, because of the very high specific impulse possible with ion propulsion, only a small percentage of the total mass of the vehicle must be propellant. This is very desirable, but if the power supply of such a vehicle is very massive there is still no room for passengers or payload. Only if the power supply is light in weight compared to the electrical power it must produce is the scheme feasible.

Studies have shown that if the power supply will furnish about 100 watts per pound of power supply, this is called its specific power, then ion propulsion is feasible and very competitive with other models.

If this specific power can be increased above this value, the performance of the vehicle looks better and better.

This, then, is our concern in the advanced propulsion work, to develop a power supply for ion propulsion having a high specific power. Solar energy, collected and converted to electricity, is feasible, but would probably result in much longer transfer times from orbit to orbit.

On the other hand, carrying chemicals for generating electrical power would result in lower overall performance than just burning these chemicals in a normal chemical rocket. Therefore, this must be a nuclear power supply.

Any of the methods of converting nuclear energy to electrical energy can, in principle, be made into a usable power supply. It has been estimated that over the next 5 to 10 years, reactors plus lightweight rotating machinery can be developed to reach specific powers of 100 watts per pound.

I believe present power supply technology would give only about 10 watts per pound.

On the other hand, the thermionic converter has one characteristic which makes it particularly suited to space application.

It will be recalled that any system converting heat to electricity must throw off a considerable amount of heat at the lower temperature of the thermodynamic cycle.

In space, this waste heat must be radiated away. The amount of energy a square foot of radiator can dissipate depends on its temperature. Doubling the temperature will increase the energy radiated by a factor of 16.

Certainly the higher the radiator temperature, the smaller and lighter the radiator has to be, in a very dramatic fashion.

In turbogenerator converters the radiator temperature cannot be very high because the turbine blades cannot stand very high temperatures.

In the plasma cell the uranium carbide temperature runs at over 2,000° C. and the collector temperature can ran at over 1,000° C.

The possibility of operating between two relatively high temperatures with the plasma cell means that much smaller and lighter radiators are, in theory, possible. The importance of this is that the weight of the radiator must be counted in as part of the power supply weight and, in fact, usually constitutes a major fraction of the total weight.

Our studies indicate that it should be possible to build a complete power supply using plasma cells with a specific power of greater than 100 watts per pound.

Every effort will be made to experimentally verify this prediction because of its great importance to space exploration.

Although very attractive, outer space applications do not exhaust the potential of the thermionic converter. The reactor described earlier consisting of a bundle of rods in a tank of water represents a concept which has a particular appeal in the maritime propulsion area and in special situations such as remote installations when such a reactor can meet reasonable lifetime and efficiency requirements.

Present reactors fall into many different categories, fast, thermal, gas cooled, water cooled, breeders, and so forth. The same variety of direct conversion reactors is in theory possible using the plasma cell in one of its many forms as the fundamental building block.

Finally, a particularly interesting application of the water moderated reactor is in commercial power production. It will be recalled that the heat thrown off by the collector of the thermionic cell is still at a relatively high temperature. This heat can be used to produce steam in the usual manner of present nuclear reactor concepts. This steam can be used to run turbines with the further production of electrical power.

This use, called a topping cycle, should push up the overall efficiency of atomic powerplants to a very interesting value in the years to come.

Representative PRICE. Thank you very much, Doctor. When do you think we will have some available hardware so we can fill in the blank on that chart in back of you?

Dr. GROVER. Our plans at Los Alamos are to spend this calendar year in an intensive survey of these reactor concepts which have been proposed. There is a wide variety possible, of course, and we want to pick one which will be reasonably easy to demonstrate as an integral reactor test of this thermocouple conversion principle. I should say that in the following year would be the design and construction of that and the test in the year following that.

Representative PRICE. How big a project do you have in this particular program?

Dr. GROVER. The program at the Laboratory has increased in the last year until it is one of the major programs of the Laboratory.

Representative PRICE. How many people are involved?

Dr. GROVER. About 60 people over the Laboratory. This is over and above the administrative support and shop personnel.

Representative PRICE. What are the funds available?

Dr. GROVER. About $1.8 million for fiscal 1961.

Representative PRICE. What is the exact status? Is it still a theoretical program?

Dr. GROVER. We feel that this year we should be able to demonstrate interesting lifetimes in reactor tests. Of course, not the years of lifetime that will be eventually required in many applications, but lifetimes long enough for use in specialized applications, such as a power supply for ion propulsion. Let us say several months. If the research program does not turn up unexpected difficulties, such things as radiation damage over that period of time, which is certainly a possible roadblock in many of the concepts that we have now, next year should see the design for an integral test reactor.

Representative PRICE. You indicated in your statement that this might be a very useful maritime propulsion system. How far away do you think you are from realizing that?

Dr. GROVER. The most important point of maritime application is the demonstration of lifetime.

Representative PRICE. Have you been able to get much data on the demonstration of lifetime?

Dr. GROVER. We measure the lifetime now in days, not due to failure of the elements, but because that is as long as the tests have been conducted. There are tests going on now in the MTR reactor, and in the Omega West, which will give us information on very long exposures suitable for year-long operation.

Representative PRICE. Mr. Hosmer.

Representative HOSMER. My question, Dr. Grover, is as to the required temperature differential between the two surfaces to which the plasma is exposed. Is the efficiency affected directly in a geometric relation?

Dr. GROVER. The maximum theoretical efficiency is governed by this temperature differential. The actual efficiency depends on several factors. One is the internal impedance of the plasma to the flow of electricity, particularly near the cold electrode. In an experiment in which we raised the temperature of the collector as high as 1,500° K. or 1,200° C., the power output of the cell remained the same because the plasma resistance went down as the voltage went up. The efficiency in that particular experiment was not measured. It was not optimized for that purpose. It is to be expected that the efficiency should drop off somewhat when you have to work at extremely high collector temperatures and consequently a smaller temperature differential between the surfaces.

Representative HOSMER. Thank you.

Representative PRICE. Mr. Westland?

Representative WESTLAND. I want to welcome another alumnus from the University of Washington. Doctor, you talk about the ratio of your achievement and hoping to get 100 watts per pound. What is the present status in that regard?

Dr. GROVER. In this particular application or over the field?

Representative WESTLAND. In this miniaturization field, what is the present status?

Dr. GROVER. Something like 5 to 10 watts per pound, I believe. This was quoted in previous testimony by Dr. Egli on the solar cells.

Representative WESTLAND. And that is what you have now with this thermionic converter?

Dr. GROVER. The actual power density in the experiments in the laboratory is much higher than that. It is in the neighborhood where, if it can be reduced to practice without serious losses, it will be in the neighborhood of well over 100 watts per pound for the complete power supply.

Representative WESTLAND. I understood you to say that you did not think you would get 100 watts per pound for another 5 years.

Dr. GROVER. In rotating machinery I think it has been estimated that one will be able to reach this goal in 5 to 10 years. I think we should be able to reach it much sooner with the plasma thermocouple.

Representative WESTLAND. What size do you hope to achieve? How many watts can you see of producing?

Dr. GROVER. There is substantially no upper limit. The lower limit would be determined by criticality considerations. In our first reactor test concepts we will attempt to build and test the smallest unit simply as a matter of economics. It still might be of the order of 300 kilowatts electrical.

Representative WESTLAND. Then there should be applications other than those you have suggested here?

Dr. GROVER. Yes. Each application would require its own separate development program to fit it into a particular area. Certainly in space we can't use water as the moderator. We would use a metal hydride. However, in the maritime application one would use water. The important unit which would be common to all in that type of reactor would be the fuel rod itself, consisting of a series of cells. This would fit quite nicely in several applications.

Representative WESTLAND. Then am I to gather that this theory is a considerable improvement over present day methods of portable reactors such as we are anticipating for the Antarctic or the Arctic?

Dr. GROVER. We believe it would be quite superior, because of its simplicity, for such remote stations. The power density is comparable or better than heat exchange reactors.

Representative WESTLAND. Do you have any sort of ETA on this project?

Dr. GROVER. No, as I mentioned, the most important thing now is to understand completely the cell and how to optimize it. How to change it to obtain the best output. We are keeping operational reactor concept considerations at a low level. We must first understand the cell completely and build and operate an integral test reactor to prove the feasibility.

Representative WESTLAND. Thank you.

Representative PRICE. Thank you very much, Dr. Grover.

Dr. GROVER. Thank you, sir.

(A paper submitted for the record on direct conversion follows:)

MAGNETOHYDRODYNAMIC POWER GENERATION USING NUCLEAR FUEL

(By Richard J. Rosa, Avco-Everett Research Laboratory, Everett, Mass.)

SUMMARY

This paper starts with a brief review of the basic theory of magnetohydrodynamic (MHD) power generators. The basic electrical properties of gases are reviewed briefly. Overall performance characteristics of MHD generators are

presented as a function of these gas properties. This gives an indication of the future potential of MHD as a conversion technique. It is pointed out that MHD shows most promise for applications where high power (1 megawatt and up) is desired.

An experimental MHD generator which has been built at the Avco-Everett Research Laboratory is described and its performance discussed.

MHD powerplant cycles are discussed. A cycle designed for use with a nuclear reactor is described. The reactor in this case would need the same temperature capability as those now being developed for nuclear rocket propulsion. Some of the possibilities for the future are discussed in which more exotic forms of high temperature reactor combined with an MHD generator may make possible nuclear powered systems having, by present standards, very low cost, high efficiency, and/or light weight.

INTRODUCTION

For the benefit of those to whom MHD is a completely new subject, it may be helpful to begin with a few comparisons with other power generating systems.

An MHD generator would not fit into a direct conversion scheme like the fuel cell, which converts chemical energy directly into electricity. Neither would it be like the thermionic or thermoelectric devices which convert heat directly into electricity.

It would be part of a system much like that used with a conventional turbogenerator. In such a system a thermal energy source heats a high pressure working fluid. The fluid then expands through the conversion machinery and is exhausted at a lower pressure and enthalpy. The conversion machine, if it is an MHD generator, delivers electrical power. If it is a turbine, it delivers mechanical power which must then be further converted into electrical power.

Figure 1 is an attempt to compare pictorially an MHD generator and a turbogenerator. The primary difference between them is that in the former the working fluid is itself an electrical conductor and forces itself through a magnetic field whereas in the latter the working fluid causes a turbine wheel to move and thereby forces a length of conducting wire through a magnetic field. In the conventional generator, currents are induced in moving copper wires. In the MHD generator, currents are induced in the working fluid itself. There are essentially no solid parts either stationary or moving directly involved in the energy conversion process.

Figure 2 shows a schematic diagram of a simple MHD generator. It consists of a duct through which the gaseous working fluid flows, coils which produce a magnetic field across the duct, and electrodes at the top and bottom of the duct. These electrodes serve much the same purpose as the brushes in a conventional generator. The gas, by virtue of its motion through the magnetic field, has an e.m.f. generated in it which drives a current through it, the electrodes, and the external load. The following three approximate equations describe the process:

$$j=\sigma(uB-E)$$

$$\text{Force}=\frac{dP}{dx}=jB$$

$$\text{Power out}=\rho u\frac{dh}{dx}=jE$$

where j=current density; σ=gas conductivity; u=gas velocity; B=magnetic field strength; E=back e.m.f. due to the load; P=pressure; ρ=density; and h=enthalpy per unit mass. Those familiar with MHD should recognize that the situation here is quite different from the situations of interest in astrophysics and fusion research essentially because the magnetic Reynolds number, in general, is less than one. (Magnetic Reynolds number, $Rm=\mu_0\sigma uL$, where μ_0 is the permeability of free space and L is a characteristic length.)

An MHD generator will have an electrical efficiency describing how much of the generated power is actually delivered to the load and how much is dissipated in its internal resistance, and it will have an effective "turbine efficiency" defined in the same manner as for a turbine.[1] It is to be noted that the energy dissipated in the internal resistance of an MHD generator is not low temperature

[1] Turbine efficiency $=\frac{h_i-h_f\ (\text{actual})}{h_i-h_f\ (\textit{isentropic})}$

where h_i=initial enthalpy and h_f=final enthalpy.

heat which must be thrown away but is high temperature heat which remains in the gas and is still available for doing work. Its dissipation has, however, created entropy and a consequent increase in pressure drop through the generator.

Other sources of loss in an MHD generator are—

1. Heat loss through the walls.
2. Electrode losses.
3. Joule losses in maintaining the magnetic field.

It seems as if all of these losses can be made reasonably small if one restricts oneself to relatively large scale devices; large more in the sense of rated power output rather than physical size. More specifically, it does not seem likely that an MHD generator will be competitive with other systems for power levels less than about 1 megawatt. It is at outputs comparable to that of commercial powerplants, i.e., 100 megawatts and up, that these devices show promise of marked superiority.[2]

The most important quantity determining the feasibility and characteristic of MHD generators is the gas conductivity. A gas will conduct if a certain, usually relatively small fraction, of its atoms or molecules become ionized, that is, lose electrons. This ionization may occur due to the absorption of several types of radiation or due to an applied electric field as in a low pressure gas discharge, or it may come about as the result of heating the gas. In the last case, which at present seems most practical, the ionization follows a mass action law as does molecular dissociation. This results in an exponential increase in ionization with temperature and a consequent exponential increase in conductivity. The conductivity is given approximately by the formula[3,4]

$$\sigma = \frac{1}{2} \frac{e^2}{mc} \frac{n_e}{n_o Q_o + n_i Q_i}$$

where, e, m, c=electron charge, mass, and mean thermal velocity; n_e, n_o, n_i= number density of electrons, neutrals, and ions; and Q_o, Q_i= effective collision cross sections of neutrals and of ions with electrons.

In almost all cases the electrons, by virtue of their higher mobility, will carry most of the current and in almost all cases one will find that $n_e = n_i$. However, Q_i is typically some thousand times greater than Q_o. As a result, when a gas approaches about 0.1 percent ionization the conductivity tends to level off and rise thereafter much less steeply as the temperature and degree of ionization increase. The important point to notice here is that when a gas is only about 0.1 percent ionized, it is very nearly as good a conductor as it would be if it were completely ionized.

Most gases such as air, CO_2, and the noble gases do not ionize appreciably until quite high temperatures are reached. If, however, a relatively small amount of some easily ionizable material such as one of the alkali metals is added, the electrical properties of the gas may be greatly improved.[5]

Figure 3 gives an estimate of the linear dimensions of a 100-megawatt MHD generator as a function of the gas conductivity and magnetic field strength. The cross-hatched regions show approximately what gas temperatures are required. With this figure we can get a rough idea of what the minimum practical value of gas conductivity and temperature is. Taking 30 feet as the maximum practical dimension for a 100-megawatt design (larger dimensions would probably result in prohibitive losses due to wall heat transfer and field coil dissipation), and 10,000 gauss, as about the largest practical field strength, leads to a minimum useful conductivity of about 1 mho per meter (10^2 mho/cm) and a minimum gas temperature of about 2000° K (about 3000° F).

Figure 4 shows a further attempt to define the characteristics of MHD generators as a function of gas conductivity and design power output. Here the power output per unit field coil mass is plotted as a function of gas conductivity for several values of design power output. Since the field coils are apt to constitute the major part of an MHD generator's weight, this plot gives an indication of the specific power output possible. Note that at high conductivities and high

[2] A. Kantrowitz and P. Sporn, Power magazine, November 1959.
[3] L. Spitzer, "Physics of Fully Ionized Gases," Interscience Publishers (1956).
[4] S. C. Lin, E. L. Resler, and A. R. Kantrowitz, "Electrical Conductivity of Highly Ionized Argon Produced by Shock Waves." Journal of Applied Physics 26, 96–109 (January 1955).
[5] R. J. Rosa, "Shock Wave Spectroscopy," "Engineering Magnetohydrodynamics." Thesis, Cornell University, 1956.

design power outputs, the specific power output may be very large. Hence, although an MHD generator must be large, it may produce a great deal of power for its size.

POWER GENERATION EXPERIMENTS

Figure 5 shows the experimental generator in operation at the Avco-Everett Research Laboratory.[6] During this run a bank of 228 50-volt light bulbs served as the load. The facility consists of an arc wind tunnel or "plasma jet"[7] mounted above a magnet. The magnet with the MHD generator channel between its poles is supported by a tank which confines the exhaust and is vented to the roof. The arc heater performs for laboratory purposes the function of the thermal energy source in a powerplant, heating the working fluid, which for most runs was argon "seeded" with potassium carbonate, to a temperature of about 3000° K. The generator itself consists of an insulated channel containing electrodes, placed between the poles of the magnet. A great many runs have been made using a variety of channel and electrode designs and under differing gas conditions. Measurements have been made of gas conductivity, electrode performance, pressure drop, and voltage and power output as a function of the load presented to the generator. Hall effects have been studied experimentally and some insight into the practical aspects of generator design has been gained. The results of these experiments may be summarized by remarking that they conform with theoretical expectations, verifying that by and large an MHD generator will perform in a thermodynamic cycle and in an electric circuit in much the same manner as a conventional turbogenerator.

MHD NUCLEAR SYSTEMS

In the preceding discussion the temperatures required for an MHD generator have been defined. These temperatures present relatively few problems in the generator itself, but they do present problems for associated components, i.e., heat sources and heat exchangers. At present, these temperatures are most readily obtained by the combustion of chemical fuels. One can imagine a furnace and generator system[5] in which the products of combustion pass directly through the generator. This avoids the necessity for having any solid heat exchanger surfaces at the peak temperatures reached by the gas.

However, the temperatures required are within the useful range of some materials, and the technology of high temperature materials is a rapidly expanding art. This gives some basis for assuming that nuclear reactors in this temperature range can be developed. In fact, the temperatures we seek are the same as those sought in the nuclear rocket (Rover) project. The recent successful test of one of these reactors was most encouraging. It is to be noted that at least for ground power generation the power density in the reactor could be very much lower than that required for a useful nuclear rocket. For ground or manned space use, one of the greatest obstacles to be overcome appears to be the problem of fission product leakage from fuel elements. It seems likely that this cannot be prevented at the high temperatures required. However, it is hoped that a satifactory scavening or cleanup system can be devised.

Figure 6 shows a simplified block diagram of a closed cycle system for use with a nuclear reactor. It is essentially a regenerative gas turbine or Brayton cycle. Because the exhaust from an MHD generator will be quite hot (around 2000° K) and contain appreciable enthalpy, the regenerator is quite desirable for high efficiency. Alternatively, one could use this heat in a steam powerplant or if compactness and simplicity were more important than efficiency, the heat might simply be dumped into the cooler. In the last case, an overall thermal efficiency of perhaps 25 percent would result.

Aside from the reactor itself, an efficient nuclear fueled closed cycle MHD system appears in many ways easier to develop than a chemically fueled open cycle system. An inert gas such as helium makes an ideal working fluid. Its lower molecular weight and its smaller electron-atom collision cross section make it possible to use lower gas temperatures than are required for combustion products. This plus the fact that the gas is noncorrosive and contains no slag allows one a greater choice of generator electrode and insulator materials and, in particular, should make heat exchangers or regenerators much easier to

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[5] A. Kantrowitz and P. Sporn, Power magazine, November 1959.
[6] R. J. Rosa, letter to editor, Journal of Applied Physics. (To be published.)
[7] T. R. Brogan, "The Electric Arc Wind Tunnel—A Tool for Atmospheric Reentry Research." ARS Journal, September 1959.

devise. The presence of small amounts of alkali metal vapor in the gas should not have a serious effect. In particular, this material will condense out in the low temperature part if the cycle and can be reintroduced to the stream just before it enters the generator, thus bypassing the nuclear reactor.

A nuclear closed cycle system of the type shown in figure 6 has been analyzed by the American Electric Power Service Corp. and the Avco-Everett Research Laboratory assuming a peak helium temperature of 2,500° K. An overall thermal efficiency of 59 percent was calculated. No attempt was made to estimate the overall capital cost of such a plant since the cost of a 2,500° K gas-cooled reactor is of course highly uncertain at present. However, it gives promise of being one of the most simple as well as most efficient nuclear reactor cycles that one could devise.

Although a system such as the above looks feasible and attractive, it may be argued that it does not take full advantage of the characteristics of an MHD generator. In this device gas enthalpy is converted into electric power without the intervention of any solid material, either stationary or moving. Of course, duct walls, field coils, and electrodes are more or less essential features of a generator, but the temperatures and stresses to which they are subjected can be quite different from those in the gas at the point where the energy is being converted. Since, however, the generator is only one component of a thermodynamic cycle, one cannot take full advantage of its characteristics unless the performance of other components, in particular the thermal energy source, are also not tied to the properties of solid matter.

A system composed of a generator working on the combustion products of a furnace (or the exhaust of a chemical rocket) achieves this objective, but is of course limited by the chemical properties of available fuel materials. It is possible that this objective can also be achieved for nuclear cycles bringing with it the exciting possibility for almost unlimited gas temperature and power density.

A cavity reactor [8-12] is a fission reactor in which all or most of the moderator is in an outer shell. The shell surrounds a cavity containing the reactant in solid, liquid, or vapor form. Since the reactant is not distributed through the moderator and can be a gas, the operating temperature is not as restricted as it is in a conventional reactor. The cavity walls must be kept below some given temperature but, just as in the case of a chemical rocket, this does not prevent the gas from being at a considerably higher temperature.

Figure 7 represents an attempt to look into the future at what a cavity reactor, and an MHD generator in a powerplant might look like. It is assumed that the working fluid consisting of helium plus about 1 atomic percent of uranium and 1 percent potassium is introduced to the cavity at a pressure of 120 atmospheres and heats itself to a stagnation temperature of 5400° K before expansion through the generator. The author is not an authority on cavity reactors but believes that reactor dimensions as sketched are more or less reasonable for the given gas conditions. The cavity is surrounded by a moderator of D_2O which weighs perhaps 7 tons. The generator is a rapidly expanding duct about 18 feet long, surrounded by a copper coil that weighs about 120 tons and requires 30 megawatts for its excitation. The generator produces 10,000 megawatts of electric power. This equals the output of 10 of the largest present-day steamplants or of over 100 conventional turbogenerators of comparable size and weight. The power density in this device would average about 30 megawatts per cubic foot, the specific power about 50 kilowatts per pound.

One can, of course, never completely avoid limitations set by solid materials. Heat transfer rates near the generator entrance would be very high, comparable to those on an ICBM nose cone. It is, however, not impossible that such rates can be handled for extended periods by water cooling or some film cooling technique.

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[8] L. A. Bell, Los Alamos Report, LA 1874 (1955).
[9] G. Safonov, Rand Report, RM 1835 (1955).
[10] S. A. Colgate and R. L. Aamodt, Nucleonics 15, August 1957. (See p. 222.)
[11] R. W. Bussard, Jet Propulsion 28, April 1958.
[12] Shepherd and Cleaver, Journal of the British Interplanetary Society, 7, No. 5, p. 185, No. 6, p. 234 (1948).

Obviously the simple system sketched in figure 7 does not constitute a complete powerplant although for some special applications it might suffice. It does give some indication of the great gain in compactness that may one day be possible in a nuclear MHD power generation system. It is evident, however, that the use of MHD for the conversion of nuclear energy into electric power depends very much on developments in reactor technology. There seems reason to hope that these developments will be forthcoming.

In conclusion an MHD generator appears to be a conversion device which can make possible very high efficiency and very high power output per pound. It is, however, a device suitable only for the production of power on a large scale. This fact would seem to make it of somewhat academic interest to the radio engineer at present. However, the need for high power levels in electronic devices is growing rapidly. The day may not be far off when such things as the transmission of large amounts of information or even power over long distances in space will call for the generation in a compact device of large amounts of electric power.

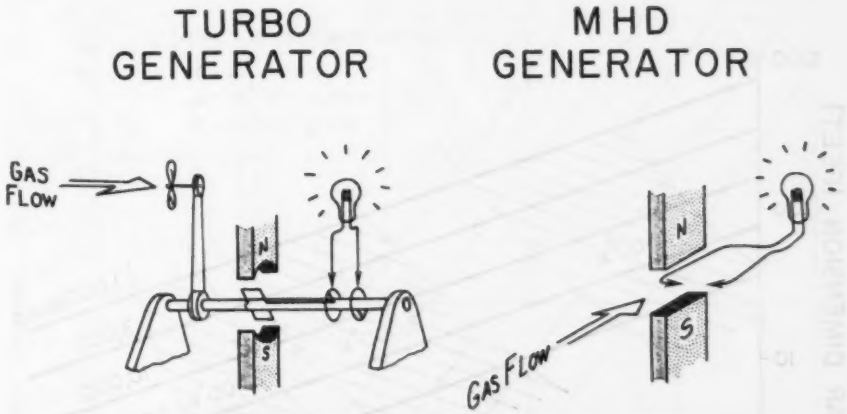

FIGURE 1.—Comparison between a turbogenerator and an MHD generator.

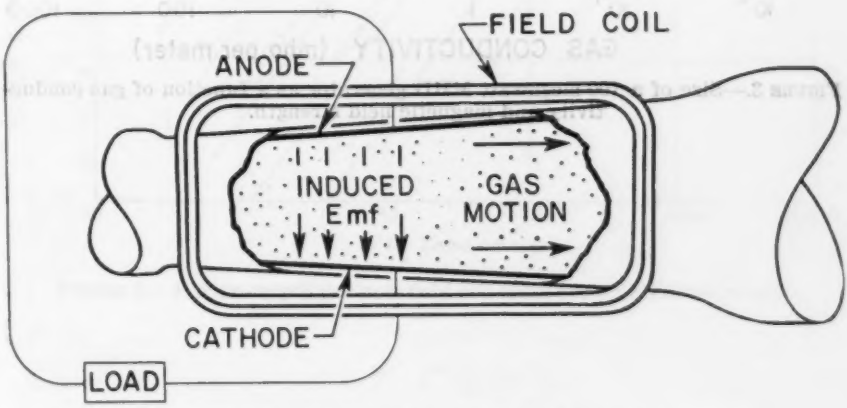

FIGURE 2.—Schematic of an MHD generator.

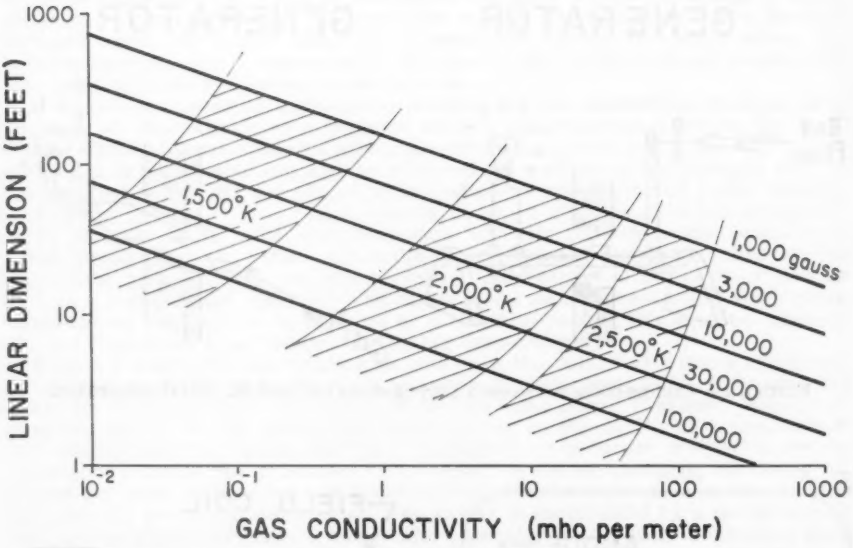

FIGURE 3.—Size of a 100-megawatt MHD generator as a function of gas conductivity and magnetic field strength.

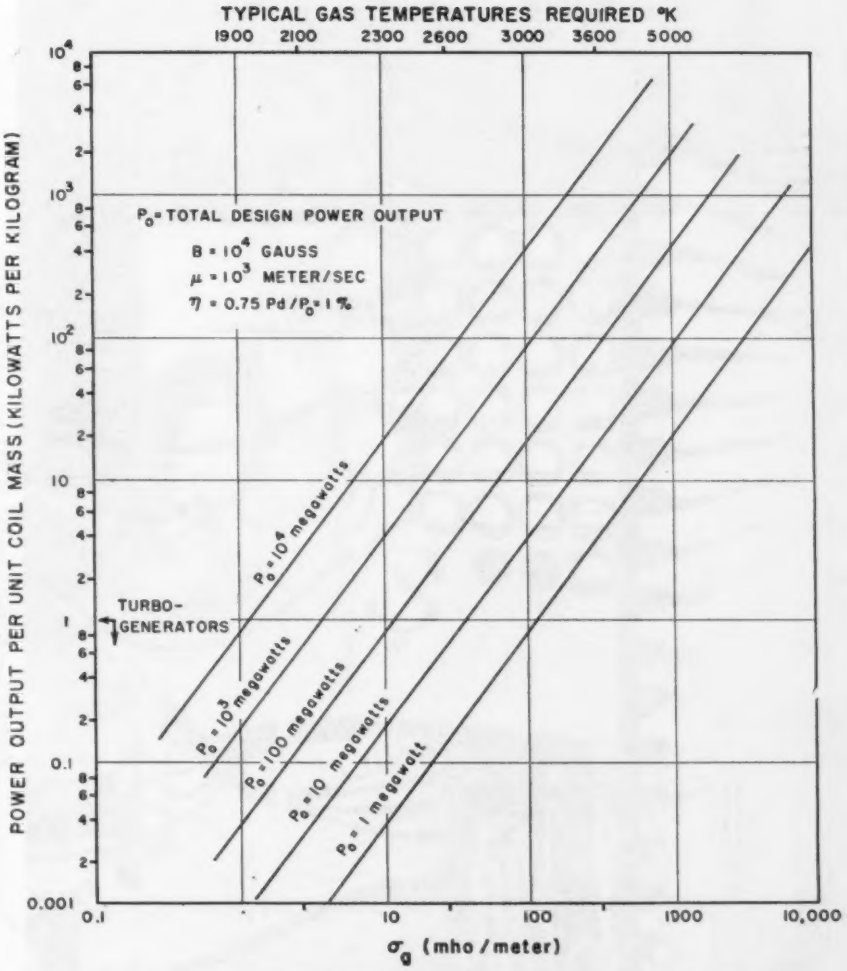

FIGURE 4.—Power output per unit field coil mass for an MHD generator.

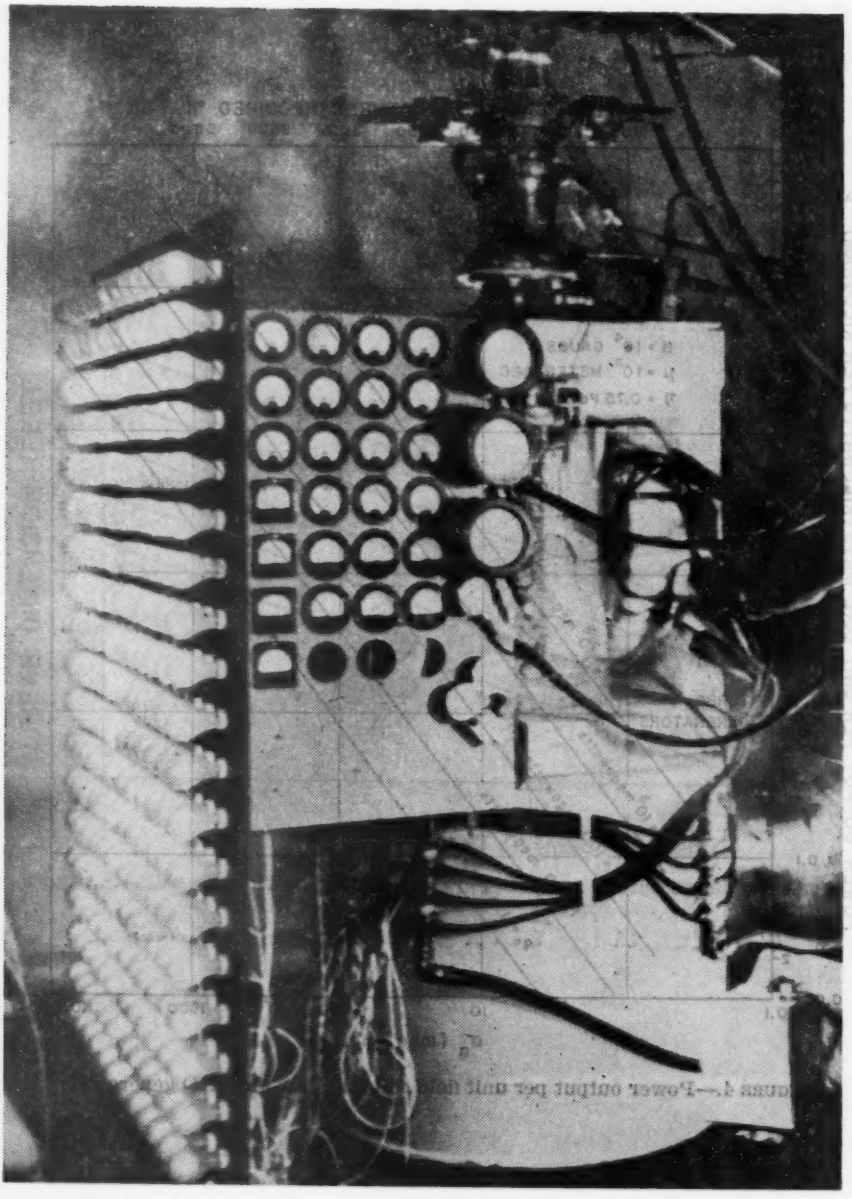

FIGURE 5.—Experimental MHD generator facility.

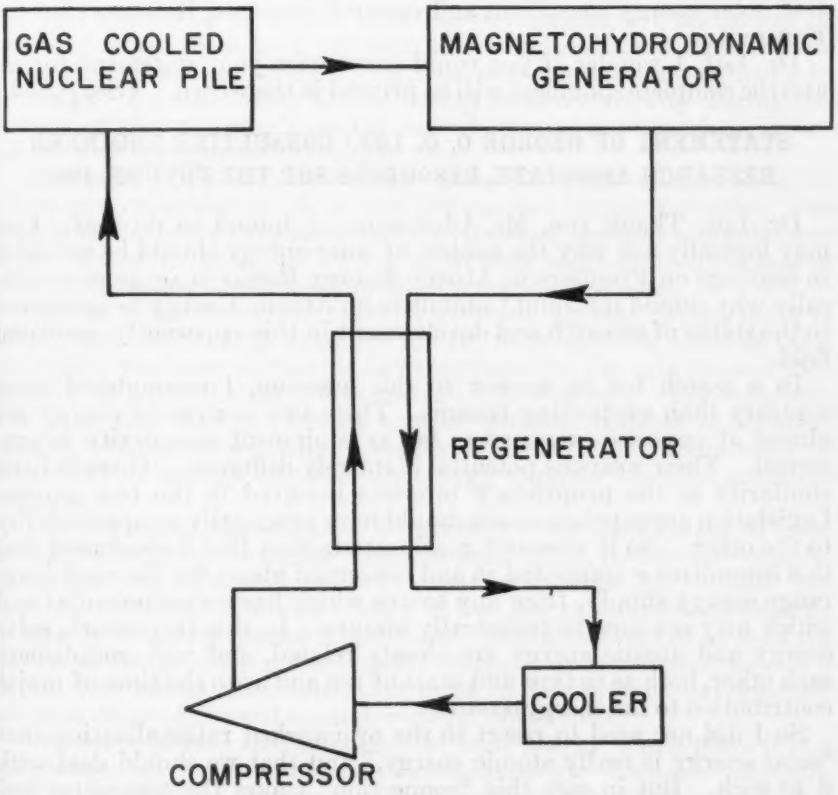

FIGURE 6.—Block diagram of an MHD nuclear closed cycle.

FIGURE 5.—Experimental MHD generator facility.

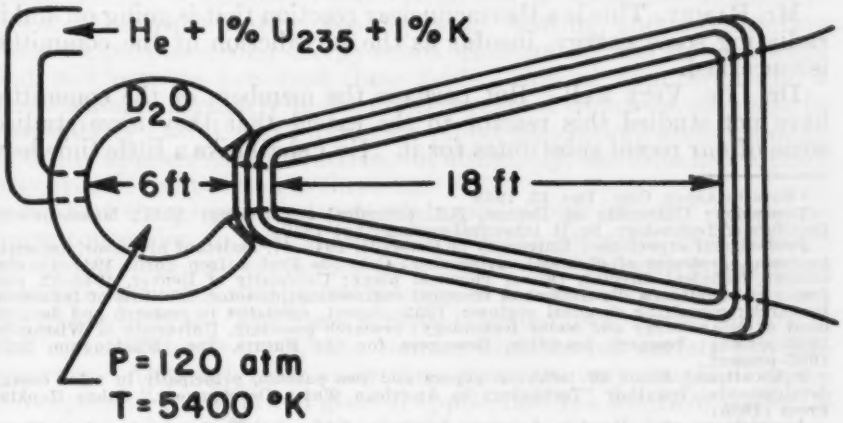

FIGURE 7.—Future possibility. A 10,000-megawatt cavity reactor MHD generator system.

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Representative PRICE. The next witness will be Dr. George O. G. Löf, solar energy consultant and research associate, Resources for the Future, Inc.

Dr. Löf, I wonder if you could summarize your statement for us, and the complete statement will be printed in the record. (See p. 359.)

STATEMENT OF GEORGE O. G. LÖF,[1] CONSULTING ENGINEER RESEARCH ASSOCIATE, RESOURCES FOR THE FUTURE, INC.

Dr. LÖF. Thank you, Mr. Chairman. I intend to do that. One may logically ask why the subject of solar energy should be included in hearings on Frontiers in Atomic Energy Research, or more specifically why should the Joint Committee on Atomic Energy be interested in the status of research and development in this apparently unrelated field.

In a search for an answer to this question, I encountered more contrary than supporting reasons. These two sources of energy are almost at opposite extremes as far as equipment complexity is concerned. Their weapons potential is entirely different. There is little similarity in the proprietary interests involved in the two sources. Legislation appropriate to one should have practically no applicability to the other. So it was with much satisfaction that I concluded that this committee is interested in and concerned about the Nation's long-range energy supply, from any source which has major potential and which may not now be technically mature. In this framework, solar energy and atomic energy are closely related, and will complement each other, both as to type and scale of use and as to the time of major contribution to the energy economy.

So I did not need to resort to the overworked rationalization that "solar energy is really atomic energy," and that we should deal with it as such. But in case this "connection" makes the committee feel more at home in the solar energy field, let me refer to the sun as a familiar, successful, self-sustaining, thermonuclear reactor, and discuss briefly the characteristics of the radiation we receive from it.

Mr. RAMEY. This is a thermonuclear reaction that is going on and is radiating some energy, insofar as the jurisdiction of the committee is concerned.

Dr. LÖF. Very well. But perhaps the members of the committee have not studied this reactor to the extent that they have studied some of our recent substitutes for it. So I shall take a little time here

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[1] Born in Aspen, Colo., Dec. 13, 1913.

Education: University of Denver, B.S. (chemical engineering) 1935; Massachusetts Institute of Technology, Sc. D. (chemical engineering) 1940.

Professional experience: University of Colorado, 1940–47, assistant professor, associate professor, professor of chemical engineering; Colorado Fuel & Iron Corp., 1947–48, consultant, technical director, Denver chemical plant; University of Denver, 1948–52, professor and chairman, department of chemical engineering; director, Institute of Industrial Research; consulting chemical engineer, 1952–present, specialist in research and development of solar energy and water technology; research associate, University of Wisconsin, 1955–present; research associate, Resources for the Future, Inc., Washington, D.C., 1955–present.

Publications: About 30 technical papers and two patents, principally in solar energy developments; coauthor "Technology in American Water Development," Johns Hopkins Press (1959).

Associations, etc.: Member, American Institute of Chemical Engineers, American Chemical Society, Sigma Xi; member, Advisory Council and Executive Committee, Association for Applied Solar Energy; member, Program Committee for World Conference on Applied Solar Energy, Phoenix, Ariz., 1955; member, United Nations International Committee on Solar Energy for 1961 World Conference; listed in Who's Who in America, Who's Who in Engineering, American Men of Science.

to outline briefly the characteristics of this radiation. I would like to ask the committee to realize that the developments I am going to summarize are those in a program almost infinitesimal in size compared to the nuclear energy programs previously discussed in these hearings. I surmise that atomic energy research activity during the period of this morning's session exceeds the solar research program which will be supported this entire year.

Although we receive solar energy at an enormous rate, about 2,000 times our requirements, it is very diffuse and highly variable. The rate is equivalent to approximately a thousand thermal kilowatts per acre. But when you deal with a small area of ground, say a square yard, this is only about one-fourth of a thermal kilowatt.

We can draw two conclusions from these figures. First, there is ample raw solar energy available for all the foreseeable needs of man. Secondly, you have to provide large surfaces for the capture and conversion of any substantial quantities of this energy. The intermittency of solar energy presents another problem in most application efforts. If continuous energy supply is needed either there must be some sort of storage or auxiliary energy supplies must be available for use.

To some extent the economic deterrents to wide application of solar energy resemble those facing atomic energy. High investment requirement per unit of energy delivery capacity is probably the most formidable problem. The raw energy is free, but the conversion expense is largely in the equipment investment. The technology of most of the processes for producing useful solar energy is extremely simple and it is not surprising, therefore, that most of the present development effort is directed toward minimizing equipment costs.

There are certain characteristics of solar energy which place it in a complementary position to that of atomic energy. Its universal availability makes it a potential supply even in the most isolated and thinly settled regions. The ease and economy of using very small solar units for heat or power make this source more attractive than atomic energy in situations where large central stations cannot be justified. For space heating, cooling, water heating, and certain other domestic purposes, solar energy seems better suited than atomic energy and will perhaps pre-empt these fields. Special applications on a small scale, particularly in the underdeveloped countries, may also be particularly useful. In large-scale power generation, atomic energy certainly appears more attractive, but ultimately, centuries hence, there may even be competition in this area.

Most of the present and potential applications of solar energy require conversion to heat. The so-called flat plate collector consists of a large blackened surface, usually metal, overlaid with one or more transparent surfaces to permit passage of the radiation and to reduce the escape of heat. The black surface is heated by absorbed radiation, and air or water is brought in contact with the hot surface for heat recovery. Although technically these units are very simple, they are too expensive for wide and general application as a source of moderate temperature heat. Most of the development is directed toward cheapening them.

The other type of solar collector is a focusing system by means of which concave reflectors receive the solar radiation and concentrate

it on a small receiver which can then be used to heat air, water, or other material. The equipment must be more or less continually moved to follow the sun and keep it in focus. Various shapes of reflectors and various materials may be used. Much of the development work connected with this type of solar heat collector is also in cost reduction.

In addition to the energy collection, since solar energy is intermittent, we usually have to provide some storage means. Most of this work has involved the solar heating of common materials such as water or rock, with subsequent transfer to other media as needed. Solar energy may also be stored in chemicals by melting them; on resolidification the chemicals return their heat to the transport fluid. Other systems are being studied but the economics of this problem are difficult.

About one-fourth of the Nation's energy consumption is for comfort heating. Since most American homes have more solar energy falling on their roofs than would be required to keep them comfortable in the winter, domestic solar heating is an extremely interesting prospect. The basic elements in a practical system are the solar collector and storage units previously mentioned, and a conventional furnace for meeting severe demands. A typical requirement for an average house in a sunny climate would be about 500 square feet of solar collector on the roof and heat storage in perhaps 1,000 gallons of water or equivalent.

But less than a dozen solar-heated buildings have ever been built. The results of these experiments have shown the feasibility of several different systems. As yet, however, none of them could be built in its present form at a cost sufficiently low to gain enough fuel savings to offset the investment. Present efforts are directed toward engineering appraisal of these systems and in developing cheaper designs. Qualified workers in the field believe that sufficient economy can be achieved, probably within this decade, to place domestic solar heating on a competitive basis with conventional fuels where there are substantial heating loads, ample sunshine, and comparatively expensive fuels.

The technology of cooling or air conditioning by means of solar energy is less advanced than solar heating. As in the case of solar heating, there is ample energy falling on the average house roof to provide comfort through the summer season also. It is fortunate that the highest cooling loads are usually coincident with the highest solar availability.

The system receiving most attention is basically a combination of the solar collector with a reasonably conventional heat-operated air-conditioning unit. Another air conditioner being investigated employs solar heat for regenerating a dehumidifying agent which is used for removing excess humidity in the dwelling space. Neither of these systems has yet been solar operated on a full scale. But it is anticipated that within the next 2 or 3 years several solar air-conditioning units will probably be tested.

Domestic solar energy utilization will probably have its first application and greatest economic attractiveness in areas where both heating and cooling are needed. The first installations will be expensive, but costs should decrease after mass production methods have been

established. If the complete solar energy system investment can be as low as $2 per square foot of solar collector in a sunny climate, this source of heat could be competitive where fuel costs more than $1 per million B.t.u.

The specific task in solar heating and cooling might, therefore, be stated as the reduction of present solar equipment costs from more than $5 per square foot of collector down toward $1 to $2. The attractive potential of solar heating and cooling can be expected to sustain interest in this development and to bring it into successful use provided that development expenses can continue to be met.

Representative PRICE. Doctor, you have a very interesting paper that gets right down to the household level. In the various experiments that we have had so far in the use of solar heating for some of our buildings, have the mechanics been very successful, aside from the cost?

Dr. LÖF. The experience has been that when these solar-heated houses started operating, they were beset with all kinds of difficulties. But over a period of a year or two these have been gradually eliminated, and I think we can say now that mechanically the systems are reliable.

Representative PRICE. So that now the problem is getting it down within the reach of those who might desire that type of heating for their home or building?

Dr. LÖF. That is right; down to where the added investment costs less than the fuel savings that you can make with it.

Representative HOSMER. In that connection, wasn't a solar house built in Denver not long ago that got a prize from the research group in Phoenix?

Dr. LÖF. That was in Phoenix, Ariz. That house was the subject of an architectural contest and was built. But through some financial problems it was never actually operated as a solar-heated house. The building in Denver is the house I am living in, and it seems to be working out pretty well.

Representative BATES. Do you have any dual systems or are these all single systems? In other words, do you ever devise a solar heating plant to supplement a conventional plant?

Dr. LÖF. Yes. I perhaps did not make that clear. All of the present experiments involve a dual system. The problem of building a solar-heated building—completely solar heated—is difficult because the solar equipment would have to be so much larger. Let me put it another way. If the solar heating system is designed to handle unusually severe spells of weather it gets disproportionately large. So it is cheaper to have an auxiliary fuel system.

Representative BATES. Do you have any system whereby you would have a direct transmission of the heat rather than through a storage facility?

Dr. LÖF. This can be done, but economically it is much more desirable to have storage. The biggest heat loads occur at night, and if there is no storage, solar energy is wasted in the daytime and there is nothing for nighttime use.

Representative BATES. You mean in an ordinary house the cost of heating that place at night is greater than in the daytime?

Dr. LÖF. Yes, in a sunny climate. Most houses with good window designs require fairly small amounts of heat during sunny days.

Mr. RAMEY. Have you looked at this house in Washington that was built in the last year? There has been a little newspaper coverage of it. It is solar heated, I believe.

Dr. LÖF. I have seen only the newspaper accounts and the photographs. I hope to see it before long.

Representative BATES. What is the maximum amount of time that you can store this heat?

Dr. LÖF. This depends entirely on how much storage capacity one is willing to pay for. Some economic studies have shown that it is probably not financially desirable to store for more than a day or two. Then, if there is a need for heat beyond that period, it is cheaper to use auxiliary conventional fuel.

Representative BATES. You mentioned about the economics of the direct conversion and its utilization. When you get into capital costs on storage you get into an economic problem, too.

Dr. LÖF. This is true. However, the cost comparison between the collector on the roof and the storage in the building is such that the collector is by far the more expensive item. The storage unit is considerably cheaper than the collector.

Representative WESTLAND. Did you not mention, Doctor, that you have 500 square feet on the roof of a house at a cost of $2 a square foot? That is only $1,000. That is not a very excessive cost. Or did I get that figure wrong?

Dr. LÖF. That is our target rather than our present cost.

Representative WESTLAND. What is it now?

Dr. LÖF. Certainly above $5 a square foot.

Representative WESTLAND. Furnace installation in a basement and all that is needed for a furnace runs pretty high.

Dr. LÖF. This is true. But don't forget you need that also with the solar system. In other words, a full capacity conventional system is required, and then the solar equipment on top of that.

Representative WESTLAND. If you get down to $2 then you are in business?

Dr. LÖF. That is what we think.

Representative PRICE. In other words, the solar system, the collector, and so forth, is only a replacement of the fuel that you use.

Dr. LÖF. That is exactly correct. You have to amortize this added investment out of fuel savings only.

Representative PRICE. Will you proceed, Doctor?

Dr. LÖF. Thank you.

The only significant domestic use of solar energy as a source of heat in this country is in water heating, principally in Florida. A simple solar heat collector about 50 square feet in area is usually mounted on the roof of the dwelling, with an insulated hot-water storage tank above it disguised as a chimney.

The water heaters in Florida are actually successful applications of solar collection systems. One of these units will supply most of the hot water requirements of an average family. This does not represent a large segment of the Nation's energy demand, but this could be a substantial solar application in terms of materials and equipment requirements.

In recent years increased interest has been shown in this country and abroad in cooking with solar energy. In contrast with the foregoing applications, this one requires considerably higher temperatures. So all of the designs involve concentration of solar energy by means of reflector systems. The most promising employs a 4-foot, dish-shaped plastic reflector, rigid or collapsible, by means of which radiant energy roughly equivalent to the heat from a conventional stove burner is concentrated onto a grill or cooking utensil. In fuel-scarce areas of the world, solar cooking may be extremely important. Dr. Duffie, whose statement follows mine, will have more to say on this subject.

Refrigeration or food cooling by means of solar energy may be classed with solar cooking in adaptability and applicability. Conventional heat-operated refrigerators as well as specially designed cooling units have been run by solar energy, and here also application in some of the underdeveloped areas looks very interesting.

I shall pass over the discussion of solar furnaces by simply mentioning these special research instruments and refer the committee to the written statement for further information.

Turning now to solar power, there are two rather distinct types of solar-power applications. One is in large-scale or central station electric power and the other involves small electric or mechanical power installations for individual dwellings, farms or very small communities. Although the two types may have some similarities, it appears that they will probably be of different basic character.

In contrast with the simplicity of obtaining heat from solar radiation, efficient generation of electricity by use of a solar-energy source is considerably more difficult. Although a flat-plate collector can be operated at temperatures higher than required for space heating, heat losses are increased and the poor turbine efficiency accompanying the use of relatively low-pressure steam makes the overall conversion efficiency 5 to 10 percent, at best. Since the raw energy is free, this low efficiency is not directly deleterious, but its indirect effect in requiring a large and expensive solar collection area is serious. An alternative is the use of large focusing systems to permit generation of high-pressure steam which can then be used at substantially higher engine efficiency, but there are optical losses in the reflector system as well as high-investment requirements in the more complicated apparatus.

Both of these methods of generating electricity from solar energy are being studied. But the cost of very large reflectors and the complexity of their operation relegate the concentrating systems primarily to the field of small power units of only a few kilowatts capacity. Larger powerplants would probably have to be composed of multiples of smaller units.

At an optimistic investment requirement of $5 per square foot of reflector surface, and assuming this figure would cover all other auxiliary costs, the 20 to 40 kilowatt-hours this surface might produce in a year in a sunny climate would have a fuel cost equivalent of over 1 cent per kilowatt-hour.

In comparison with average fuel prices of 0.3 to 0.4 cent per kilowatt-hour generated in the United States, these figures show that large-scale production of electricity from solar energy by means of

focusing systems is in the distant future. Some development work is being done along these lines, but the principal efforts with focussing systems are now directed toward small-scale power generation.

Some preliminary economic studies of the large-scale generation of electric power by use of flat-plate collector designs as now known have shown that amortization of the required investment would cost several cents per kilowatt-hour generated. Most of these evaluations have not included any form of energy storage, so power would be available from such systems only during sunny periods.

Hence, the value of this power would be equivalent only to the cost of fuel saved by a companion plant of full capacity. Estimates have shown that U.S. fuel prices would have to rise two or three fold before electricity generated from solar energy by such means could be significant in this country.

Another approach to the problem has recently made economical large-scale solar power appear more promising. Research in Israel and elsewhere has shown that artificial water ponds of moderate depth, when protected from excessive heat loss by use of transparent covers, or by reducing thermal convection in the water, can be solar heated to temperatures adequate for power generation from the resulting low pressure vapor. Although efficiencies at these temperatures are low, a very cheap "boiler" such as a simple pond of water, with minimum added investment for heat loss reduction, might permit power generation at less than a cent per kilowatt-hour. Such a plant should be able to supply firm power because of the inherent thermal storage feature. Research on materials of construction, techniques of operation, and other factors must go considerably further before the potential of this concept can be appraised.

B. SMALL-SCALE POWER GENERATION

In this discussion small scale may be considered a power-capacity range of a few watts to a few kilowatts. In use, these power generators would usually be operated by the consumer, no transmission of electricity being involved. This immediately suggests the use of such systems in areas where electricity is either not available or may be supplied by transmission line only at very high expense.

1. Photoelectric generators: One of the most spectacular applications of solar energy in recent years is the so-called solar battery. This usually consists of a metallic silicon wafer of high purity, with traces of other elements to give it the property of a semiconductor. When exposed to solar radiation, the best of these cells can convert up to 15 percent of the radiant energy to electricity, and even the average cell is now capable of nearly 10 percent efficiency.

A number of small-scale uses of solar cells have already been introduced, and others are being developed. Although silicon cells have comparatively high efficiency, they are costly per unit of electric capacity. One kilowatt of daytime capacity requires about 100 square feet of silicon cells at an investment of over $100,000. The applicability of this device appears now to be limited to small electrical loads, such as in communications equipment. Use in radio, telephone, signaling, and other devices is growing. The most spectacular demonstrations have been the powering of radio transmitter

equipment on satellites and space probes. Where the cost of this power supply is such a minor part of the entire investment, this method of furnishing small amounts of electric power appears very attractive.

Much research is being directed toward increasing cell efficiency and reducing costs. Although improvements can be expected, tremendous differences are not foreseen. Therefore, solar power from silicon cells is likely to remain expensive and of special applicability. Other photoelectric materials may come into use, and possibly at lower cost, so the potential of this development might increase.

2. Thermoelectric generation: Dramatic progress has been made in the last 3 years in the conversion of heat to electricity by means of the thermoelectric principle, that is, the supply of heat to a junction of two different conductors, thereby causing a current to flow through a complete circuit. Although this principle has long been known, only recently have new materials been tried and conversion efficiency increased from the fraction of 1 percent formerly achieved.

Although conventional heat sources have been involved in most of the development work, there is every reason to expect that concentrated solar energy will be useful in supplying thermoelectricity in remote areas. At temperatures near 1,000° F. the efficiency of the new thermoelectric systems can be as high as 10 percent. When fully developed, the thermoelectric elements are expected to be comparatively cheap per unit of electrical capacity. With no moving parts and practically unlimited life, these units, in combination with cheap plastic reflectors, may be of exceptional importance in supplying up to a few kilowatts for pumping water, lighting rural buildings and other uses. Discussion of this potential in the underdeveloped countries is presented by Dr. Duffie.

3. Thermionic power generators: Recent experiments have indicated that heat can be converted to electricity in an electronic tube where very high temperature heating of a cathode causes a flow of electricity through the tube to an anode and an external circuit. The placement of the cathode at the focus of a precise solar reflector can accomplish the purpose. Efficiencies in the 20-percent range may be possible in such a system. Although the highest desired temperatures have not yet been attained in these tubes, and if solar operated they will require high precision in the reflector, the potential nevertheless is interesting enough to justify further development.

4. The hot-air engine: Although the hot-air engine—sometimes known as the Stirling engine—was discarded generations ago and replaced by the reciprocating steam engine and later by the steam turbine, it might again become useful in modified form. Recent developments abroad have led to major reductions in the bulkiness of the equipment and may permit practical operation in a solar concentrating system. By use of air rather than steam and alternately heating and cooling it by movement back and forth within the engine itself, pressure rises and falls in the cylinder, thereby moving a piston. Comparatively high efficiency can be achieved in an engine of this type if temperatures are adequate.

Experiments are proceeding in several laboratories not only with improvements in the engine itself, but with combinations of focusing solar reflectors and small hot-air engines. A reflector of about 100

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square feet could possibly develop 1 horsepower, and with further improvement perhaps as much as a kilowatt.

C. AUXILIARY POWER IN SPACE

The only available energy in space in any substantial quantity is solar energy. In the general vicinity of the earth's orbit its intensity is nearly half again as large as on earth. The powering of communications equipment and instruments on satellites and other space vehicles by means of solar energy has already been successfully demonstrated with the silicon cell. With the establishment of more permanent and perhaps manned space stations or planetary based outposts, power requirements will grow substantially. The continuous solar energy supply—except when in the shadow of celestial bodies—the lack of air to impede or destroy lightweight structures, the lack of any radioactivity or other hazardous byproducts, make solar energy a strong contender for filling such requirements.

Competitors of the silicon cell are thermoelectric and thermionic converters and even reasonably conventional vapor or hot air engines all in conjunction with focusing solar reflectors. With an average 10 percent overall conversion efficiency, solar concentration areas of 60 to 70 square feet per kilowatt of capacity would be required. The transport into space of large solar reflectors in a folded or deflated form, inside a compact and lightweight package in a rocket, appears entirely feasible. These systems for supplying auxiliary power in space are receiving much consideration. It can be expected that further spectacular developments will soon be forthcoming in these new and challenging areas.

D. LONG-RANGE RESEARCH IN SOLAR POWER

A tantalizing goal in the field of solar energy conversion has long been the artificial duplication or improvement on natural photosynthesis. By means of processes not fully understood, living plants can convert solar energy to chemicals, store them, and make them available for subsequent use. With the exception of our nuclear fuels and hydroelectric power, all of our energy economy is based on the occurrence of this process in past geologic ages of the earth. The efficiency of converting solar energy to chemical energy by natural processes is extremely low, probably averaging less than 1 percent. Moreover, the products of the conversion usually have inconvenient bulk, for example, leaves, wood, et cetera.

If a chemical reaction or combination of reactions could be found whereby solar energy could be efficiently captured for an indefinite length of time, then the process could be reversed and the energy released at will.

Photochemists have been working in this field for years, and several interesting possibilities have developed. But the energy conversion efficiencies have so far been extremely low, in fact, even below those of natural processes. Only the ultraviolet portion, or highest energy part of the solar spectrum, has been successfully utilized in such reactions as water decomposition into hydrogen and oxygen.

Another, perhaps even more interesting prospect is for sunlight to cause a chemical reaction which on reversal will yield electricity

directly. A photogalvanic process of this sort would be carried out in an electrolytic cell, where a reaction would go one way when exposed to sunlight, and when darkened, go in the other direction to yield an electric current. Only the most limited progress has been made in this field, and much further work is required before its potential can be appraised.

These fundamental research studies may prove to be of great practical value if some fortunate factors materialize. Not only the conversion of solar energy to electricity, but also its intermediate storage would then be accomplished in one overall process.

It must be emphasized that there are speculative and long-range potentials, with no firm assurance of realization. But in comparison with thermonuclear energy, described elsewhere in these hearings, this potential cannot be considered less remote nor less important.

III. SPECIAL APPLICATIONS OF SOLAR ENERGY

Several present and potential uses of solar energy involve neither direct heat production nor power generation. Instead, some mineral or agricultural product is involved.

A. Sea water demineralization: One of the promising processes for low cost sea water demineralization to produce domestic and industrial fresh water supplies involves very simple, direct utilization of solar energy in the evaporation of sea water from shallow artificial basins. If these basins are provided with sloping transparent covers, the water vapor condenses on their undersides and runs into edge troughs and to storage. Pilot plant experiments are in progress in Florida under the auspices of the Office of Saline Water of the U.S. Department of the Interior. Several solar distiller designs, including asphalt basins, glass covers, and all-plastic units, are being tested. Capable of producing nearly 5,000 gallons per sunny day per acre, these systems involve costs almost exclusively the amortization of the original investment. Much effort is being devoted to the development of designs of exceptional economy in materials and fabrication.

In arid regions where coastal deserts are common, ample solar energy is usually available and fuels may be costly. Solar distillation of sea water should become of considerable importance in the years ahead, as water demand and price rise sufficiently to justify the greater cost of this demineralized supply. Development of new and simpler designs and processes are steadily leading toward practical utilization of this system for augmenting water supplies.

Representative WESTLAND. I am quite interested in the saline water program. I thought you got beyond the pilot plant stage on this, Doctor.

Dr. LÖF. A number of processes are beyond the pilot plant stage, but the solar demineralization process is still at that point.

Representative WESTLAND. Have you gotten into any area of figuring the cost per thousand gallons of fresh water?

Dr. LÖF. Yes. These figures are largely dependent upon what scale or what plant size is being considered.

Representative WESTLAND. Let us talk about a million gallons a day.

Dr. LÖF. On this basis the best estimates for solar demineralization fall in the range of a dollar per thousand gallons.

Representative WESTLAND. Perhaps it is other systems that have gotten us down to around 50 cents, I believe.

Dr. LÖF. These have yet to be demonstrated. I am not close enough to the other processes to comment on them in detail, but I certainly believe that solar demineralization is going to be competitive in many areas and preeminent in areas where fuel costs are high.

Representative WESTLAND. Can you possibly foresee a 50-cent cost on a million gallons a day?

Dr. LÖF. I certainly don't believe it is out of the question. I don't believe we have any designs now which would permit a sufficiently low investment to make that possible. There are some estimates from one of the private companies working with plastic films that show costs in that range, 50 cents a thousand gallons, if the life of the plastic is as long as hoped for.

Representative WESTLAND. To amortize them?

Dr. LÖF. Yes.

Representative WESTLAND. I think this is an extremely important field and I am happy to see that it is progressing.

Dr. LÖF. Thank you.

Representative BATES. Doctor, what is the cost of the evaporating systems through condensation? Do you have any figures on that at all?

Dr. LÖF. The multiple effect evaporation systems using oil fuel.

Representative BATES. Triple effects?

Dr. LÖF. In Aruba in the Caribbean, Gulf of Mexico, the reported cost is around $2 per thousand gallons.

In the demonstration plant which the Office of Saline Water is going to build in Texas, I believe the cost is estimated below a dollar or in the dollar range.

B. Salt production: Salt production by solar evaporation of sea water and salt lakes is a practical solar energy application of long standing. It is mentioned here primarily as an illustration of a mature solar energy application, in contrast with most of the other uses which are in various stages of development.

The world's solar salt production is in the millions of tons per year, and various other chemicals such as soda ash, borax, and potash are obtained as byproducts. Most of the U.S. production is on the California coast and the Great Salt Lake of Utah.

C. Algae production: A considerably different type of solar energy application is the growth of algae—simple waterborne plants—for animal and human foods and as sources of chemicals. In the presence of carbon dioxide and water, algae can convert solar energy to carbohydrates and proteins at efficiencies considerably higher than can ordinary land crops.

Although the economics of the process are currently not as attractive as most land-based agriculture, any future shortage of land for crop production could be offset by this alternate food source. Development of this process is now concentrated in Japan.

D. Solar drying: Although the sun has been used for centuries in the drying of crops, fish, and meats, there are potentials for more efficient and general use of solar energy for drying purposes, particularly when augmented by modern equipment. For preservation of perishables such as livestock feed, fruits, and vegetables, meats, and

the like, for product treatment as in coffee drying and dehydrated food production, and for raw material preparation as in coal and oil shale drying, solar energy use may become increasingly important. Both for certain unique advantages of cleanliness, and also for the fuel savings possible, solar drying is of interest.

A recent study of the solar drying of water-saturated Brazilian oil shale indicates, for example, that the overall economics of the entire oil-production enterprise might be affected by the feasibility of this preliminary drying step.

IV. CONCLUSION

In a Nation as rich in energy resources as the United States, the need for applying solar energy to any of our conventional requirements appears quite remote. The same might be said of atomic energy. But there are two or three aspects of this question which make this first view not entirely valid.

First, if the cost of these alternate supplies can be reduced to the point where they are better than competitive with existing sources, there is every reason to develop and utilize them.

Secondly, departing from the limitations of the present and looking to the welfare of future generations, conservation of high value resources for other uses by development of these alternate supplies may well be a desirable current policy. The hydrocarbons, for example, must serve as raw materials for many types of products, chemicals, clothing, building materials, and a host of other products.

And, finally, in the world view, solar energy is actually needed now in many places on earth. The economic potential and the technical background for developing these sources does not exist in most of those regions, so countries having such potential, and particularly the United States, must be looked to for these developments. Solar energy falls clearly into this category; its technology would be a most valuable export. Dr. Duffie will emphasize the value of this policy in his testimony.

In closing, let me state my opinion that in proportion to the very modest expenditures, development of solar energy for man's use has shown remarkable progress. But progress could be far more rapid, were funds and facilities available. In photochemical research, for example, the long sought breakthrough might come much sooner. With technical and economic prospects no less attractive than many of the well supported atomic power studies, but with perhaps only one thousandth the funds, solar energy research and development deserves substantially greater support.

Representative PRICE. Doctor, you feel, then, that the state of this program of research in solar energy has reached the point that with greater financial support and effort you could step up progress in the program?

Dr. LÖF. Yes; I believe that, sir.

Representative HOSMER. It is not just a matter of more funds, is it? You have to have some reasonable direction on the research that is to be done.

Dr. LÖF. That is certainly true, sir.

Representative HOSMER. But the Egyptians in the days of the Pharaohs were working on this problem. The man who invented the first ironclad warship spent the rest of his life thereafter on this.

Today we have, from what you have described so far, a number of interesting ideas that are being pursued on at best a haphazard basis. For about 5 years now I have had a bill in trying to get somebody interested in having a $1 million a year program for 10 years on solar energy research set up essentially in the manner that the saline water research program has been set up.

Would that method, in your opinion, be a productive way of going about the additional expenditures you have recommended?

Dr. LÖF. I believe that the stimulation of these additional funds that you have proposed would be highly beneficial to the progress of solar energy research and development, and that we need not worry about the question of whether there will be manpower available.

There are many qualified people who I am sure would want to be involved in a program such as you mention. I believe the greatest needs are probably in the fields of basic investigation such as the photochemical process and some of the new direct conversion processes.

As a matter of fact, solar energy has sort of been riding along on the coat-tails of a number of these atomic energy developments. The thermoelectric developments we heard about today, for example, we think will be very useful in solar energy research and development, but the solar research people had nothing to do with making them.

I certainly feel that not only the program that you have been attempting to initiate, but also the catalytic effect that it would have on further industrial and foundation support would all be very beneficial.

Representative HOSMER. As a matter of fact, it is attractive and understandable to talk about such things as heating houses and so on, but the direction of the effort now needed is in space types of investigations and not earthbound applications.

Dr. LÖF. I think a combination ought to be envisioned. Just which funds are to be used for what purpose is another matter. Probably the industrial money ought to go into applications.

Representative HOSMER. You can throw out solar cells because there is a good deal of industrial effort on that now.

Dr. LÖF. That is right.

Representative HOSMER. But photosynthesis and even production of biological products and so forth, probably because of the interruptibility of this source, is something that should deserve a great deal of emphasis.

Dr. LÖF. I believe that is right.

Representative HOSMER. Thank you.

Representative PRICE. Mr. Ramey.

Mr. RAMEY. In that connection, what is the general dollar amount being expended on solar energy development now? Can you give us any estimate?

Dr. LÖF. Expenditures on actual solar research and development as such, not including miscellaneous activities such as the use of solar furnaces to study high temperature resistance of materials—which after all is only incidental to the development of solar energy—but just the development of solar heat and power, I am sure is well under $1 million per year. I think that would include the world effort. A few hundred thousand would cover the U.S. effort.

Mr. RAMEY. I have heard it often speculated if the Government had put $1 billion of the funds or the money it had put into atomic

energy research and development into solar development, you might have a different picture on solar energy right now.

Dr. LÖF. I am sure you would.

Representative PRICE. Thank you very much, Doctor, for a fine presentation.

Dr. LÖF. Thank you sir.

(Dr. Löf's formal statement follows:)

DEVELOPMENT OF SOLAR ENERGY FOR INDUSTRIAL AND DOMESTIC USES
BY GEORGE O. G. LÖF

One may logically ask why the subject of solar energy should be included in hearings on "Frontiers in Atomic Energy Research." Or, more specifically, why should the Joint Committee on Atomic Energy be interested in the status of research and development in this apparently unrelated field?

In a search for an answer to this question, I encountered more contrary than supporting reasons. These two sources of energy are almost at opposite extremes as far as equipment complexity is concerned. Their weapons potential is entirely different. There is little similarity in the proprietary interests involved in the two sources. Legislation appropriate to one would have practically no applicability to the other. So it was with much satisfaction that I concluded that this committee is interested in and concerned about the Nation's long-range energy supply, from any source which has major potential and which may not now be technically mature. In this framework, solar energy and atomic energy are closely related, and will complement each other, both as to type and scale of use and as to the time of major contribution to the energy economy.

So I did not need to resort to the overworked rationalization that "solar energy is really atomic energy," and that we should deal with it as such. But in case this "connection" makes the committee feel more at home in the solar energy field, let me refer to the sun as a familiar, successful, self-sustaining, thermonuclear reactor, and discuss briefly its characteristics. I do this in the belief that perhaps the committee has not been as fully acquainted with this source of energy as with man's recent substitutes for it. After this introduction, I should like to outline some of the more promising uses of solar energy and the progress being made toward their development.

Although solar energy is received by the earth at an enormous rate (in the United States about 2,000 times our total energy requirement), it is of very low concentration and of great variability in time and place. The quantity may be visualized by noting that the average acre of land in the United States receives about 60 million B.t.u. of radiant energy per average day, and that in the sunniest regions, summer normals exceed 100 million B.t.u. per day per acre. These rates are roughly equivalent to 24-hour averages of 700 to 1,200 thermal kilowatts per acre. In contrast with these rather impressive totals based on an acre of ground, the figures involving, say, a square yard of surface illustrate the diffuseness, or low concentration of solar energy. Only about 25,000 B.t.u. are received on such a surface during a sunny day, or about one-fourth of a thermal kilowatt on a 24-hour basis.

Two conclusions may be drawn from figures such as these. First, there is ample "raw" solar energy for all the foreseeable needs of man, even if no other sources were available, and second, large surfaces must be provided for capture of substantial quantities of solar energy and its conversion to useful forms. The need for large solar intercepting areas should be clearly realized, for it is impossible to increase this solar energy rate by any artificial means. Even solar magnifiers or concentrators can deliver energy only in direct proportion to the area they occupy.

The intermittency of solar energy presents another problem in most application efforts. Not only are there the regular day-night and season-to-season cycles, but there are the effects of irregular variation caused primarily by clouds. If continuous energy supply is required, either some sort of storage must be employed, or some other energy source has to be utilized during periods of insufficient sunshine.

To some extent, the economic deterrents to wide application of solar energy resemble those facing atomic energy use. High investment requirement per unit of energy delivery capacity is probably the most formidable problem. In

the operation of solar energy systems, there is very little cost above the first cost and its amortization. The "raw" energy is free, and conversion expense is almost entirely in equipment investment. Since the technology of most processes for producing useful energy from solar radiation is comparatively simple, it is not surprising that much of the present development effort is directed toward minimizing equipment costs.

As indicated later in greater detail, there are certain characteristics of solar energy which place it in a position complementary to that of atomic energy. Its universal availability makes it a potential supply even in the most isolated and thinly settled regions. The relative ease and economy of using very small solar units for heat or power may make this source more attractive than atomic energy in situations where large central stations cannot be justified. For space heating, cooling, hot-water heating, and other domestic purposes, solar energy seems better suited than atomic energy and will perhaps preempt these fields of use. Special applications on small scale, particularly in the underdeveloped countries, for water pumping, sea-water demineralization, crop drying, cooking, and refrigeration should fall to solar energy. In large-scale power generation and in transportation, atomic energy appears more attractive; but ultimately, these applications may become areas of competition between the two sources.

I. RESEARCH AND DEVELOPMENT IN OBTAINING HEAT FROM THE SUN

A. Solar energy converters or "collectors"

Most of the present and potential applications of solar energy require the conversion of solar radiation to heat. Two general types of equipment can be used. The so-called flat-plate collector consists of a large blackened surface, usually metal, covered with one or more transparent surfaces which permit passage of solar radiation and reduce the escape of heat radiation. The black surface absorbs the radiation, becomes heated, and air, water, or other fluid circulated in contact with the hot, black surface recovers the heat for subsequent rise. Although technically very simple, these units are now too expensive for wide and general application of a source of moderate temperature heat. Most of the development work in flat-plate solar collection systems is therefore directed toward reducing the cost of the apparatus. Some of the principal efforts now being made are the development of designs and techniques which can lead to factory production of cheap, prefabricated solar collector units, the evaluation of inexpensive plastic films as transparent surfaces, and the development of special heat absorbing surfaces and treated transparent surfaces for minimizing heat losses.

The other type of solar collector is a focusing system by means of which a concave reflector receives the solar radiation and concentrates it on a small receiver which can in turn be used to supply heat to air, water, or other material. This equipment must be more or less continually moved to follow the sun and keep the receiver in focus. Various shapes and construction materials can be used for the reflectors, e.g., trough- or round-shapes of metals, plastics, glass. Lenses may also be employed, but in general they are more expensive than the simpler reflectors. Much of the development work connected with this type of solar heat collector is also oriented toward cost reduction.

B. Heat storage developments

Since solar energy is intermittent, much effort has been devoted to methods for its storage so that it may be used during sunless periods. Most of the work has involved solar heating of common materials, such as water and rock, and subsequent transfer to other media for use as needed. A substantial drawback is the large volume and weight of storage material required per unit of heat storage capacity. Heat may also be stored in certain chemicals by melting with solar heat. On resolidfication, the molten material gives up its heat of fusion to air or other heat transport media. Efforts are being directed toward efficient and economical heat storage means, both for comparatively low temperatures and for high temperatures, but the problem is difficult. Although there is interest in developing heat storage materials, not only for solar heat applications but also for off-peak electric house heating, progress has been slow. It appears that more effort could well be devoted to this problem.

C. House heating and cooling

About one-fourth of the Nation's energy consumption is for comfort heating. Since most American homes have more solar energy falling on their roofs than

would be required to keep them comfortable in the winter, domestic solar heating is an extremely interesting prospect. The basic elements in a practical system are the solar collector and storage units previously described and various auxiliaries such as a conventional furnace for meeting severe demands, control equipment, pumps and blowers, and the like. Typical requirements for an average house in a sunny climate should be about 500 square feet of solar collector and heat storage in 1,000 gallons of water or equivalent.

Less than a dozen solar-heated buildings have been built in the United States. The results of these experiments have shown the feasibility of several somewhat different systems. As yet, however, none of them could be built in its present form at a cost sufficiently low to gain enough fuel savings to offset the added investment expense. Present efforts, therefore, in addition to engineering appraisal and evaluation of existing solar-heating systems, are in developing more economical, prefabricated designs. Qualified workers in the field believe that sufficient economy can be achieved, probably within this decade, to place domestic solar heating on a competitive basis with conventional fuels in favorable locations, i.e., where there are substantial heating loads, ample sunshine, and comparatively expensive fuels. Industrial participation in practically all of the present solar house-heating development projects is an indication of ultimate commercial potential. Introduction of solar house heating will undoubtedly be "spotty" at first, gradually enlarging in areas roughly south of the 40th parallel (San Francisco, Denver, Cincinnati, and Washington, D.C.). As technology and economics improve, and if conventional energy costs rise, gradual entry into the more northerly areas can be expected.

The technology of cooling or air conditioning by means of solar energy is less advanced than solar heating, but this application is receiving considerable attention at the present time. As in the case of solar heating, there is ample energy falling on the average house roof to provide comfort through the summer season also. It is, of course, fortunate that the highest cooling loads are usually coincident with highest solar availability. Two systems are being investigated. The one receiving most attention in several university and industrial laboratories is basically a combination of a solar collector with a reasonably conventional, heat-operated air conditioning unit. (Two fuel-operated domestic air conditioners of this type are on the market.) Another air conditioner being investigated employs solar heat for regenerating a dehumidifying agent which is used for removing excess humidity in the dwelling space. Neither of these systems has yet been solar operated on full scale. It is anticipated, however, that within the next 2 or 3 years, probably several solar air conditioning units will be tested.

Domestic solar energy utilization will probably have its first application and greatest economic attractiveness in areas where both heating and cooling are needed. By this joint use, the solar energy collection and storage system can effect the greatest savings in conventional energy costs and thereby pay for itself in the least time. The first installations will be expensive, but costs should decrease after mass production methods have been established. If the complete solar energy system investment can be as low as $2 per square foot of solar collector, in a sunny climate, this source of heat could be competitive where fuel costs more than a dollar per million B.t.u. delivered (roughly equivalent to 75 cents per 1,000 cubic feet of natural gas or 10 cents per gallon of fuel oil). However, at the present time, a solar collector and associated equipment cost several dollars per square foot. The specific task in solar heating and cooling might therefore be stated as the reduction of solar equipment costs from more than $5 per square foot of collector down toward $1 to $2 per square foot. The attractive potential of solar heating and cooling can be expected to sustain interest in this development and to bring it into successful use provided that development expenses can continue to be met.

D. Hot water heating

The only significant use of solar energy as a source of heat in the United States is in domestic water heating, principally in southern Florida. Simple solar heat collectors about 50 square feet in area are usually mounted on the roof of the dwelling, with an insulated hot water storage tank above it disguised as a chimney. Natural circulation of water through the collector during the day absorbs solar heat and stores it for subsequent use as hot water. Such a unit will supply most of the hot water requirements of an average family.

The advent of cheaper domestic fuels in areas where solar water heaters have been commonly used has caused a decline in their use in this country. However,

substantial application in Japan and Israel is reported. The use of cheaper materials and prefabricated construction, coupled with possible increases in domestic energy costs, should result in increased future use. Domestic water heating does not represent a large segment of the Nation's energy demand, but this could be a substantial solar application in terms of materials and equipment requirements.

E. Solar cooking and solar refrigeration

Increased interest has recently been shown in this country and abroad in cooking with solar energy. In contrast with the foregoing applications, this one requires considerably higher temperatures; all of the designs therefore involve concentration of solar energy by means of reflection. The most promising employ 4-foot dish-shaped plastic reflectors, rigid or collapsible, by means of which radiant energy roughly equivalent to the heat from a conventional stove burner is concentrated onto a grill or cooking utensil.

Except for outdoor recreational use, solar cooking appears to have no potential in the United States because of its inconvenience, lack of dependability, and the rather insignificant cost of energy for conventional cooking. In fuel-scarce areas of the world, however, solar cooking may be extremely important, but I shall omit further discussion in favor of more specific information presented elsewhere in these hearings.

Refrigeration or food-cooling by means of solar energy may be classed with solar cooking, in adaptability and applicability in the United States and abroad. Conventional, heat-operated refrigerators as well as specially designed cooling units have been run by solar energy. However, the very minor energy savings in the United States, along with certain inconveniences and difficulties, make this application of rather academic interest in this country. Here also, however, applications in the less developed countries have sufficient promise to justify special discussion by another witness before this committee.

F. Solar furnaces

By means of segmented reflectors many feet in diameter, large amounts of solar energy can be brought to a small and concentrated focus, at temperatures in excess of 5,000° F. To achieve such temperatures, high manufacturing precision must be used. For this reason, these installations are very expensive per unit of heat delivery capacity. They have unique applicability and usefulness in metallurgical and ceramic research, particularly in the study of the properties of materials at very high temperatures. High temperatures can also be achieved in much smaller solar furnaces of excellent optical characteristics, such as obtained with large searchlight mirrors, but the quantity of heat available at the focus of these units is limited. The largest solar furnaces are located in southern France, Algeria, and recently, at the Quartermaster Research and Development Center in Massachusetts.

Although there is a possibility that these units might be used as sources of heat in high temperature industrial processes, it is doubtful that the economics will ever justify this application. Electric furnaces should be able to meet the requirements at considerably lower cost. The solar furnace will probably remain primarily a useful research tool.

II. SOLAR POWER

In contrast with atomic energy, the near future potential applications of solar energy appear to be more along the lines of heat than power. However, considerable research and development are being devoted to solar power processes, and in the long view, electricity and other forms of work will be produced from solar energy in substantial quantity.

There may be two rather distinct types of solar power applications. One is in large scale or central station electric power and the other involves small electric or mechanical power installations for individual dwellings, farms, or very small communities. Although the two types may have some similarities, it appears that they will probably be of different basic character.

A. Large scale solar power

In contrast with the simplicity of obtaining heat from solar radiation, efficient generation of electricity by use of a solar energy source is considerably more difficult. Although a flat-plate collector can be operated at temperatures higher than required for space heating, heat losses are increased, and the poor turbine efficiency accompanying the use of relatively low pressure steam makes the

overall conversion efficiency 5 to 10 percent, at best. Since the raw energy is free, this low efficiency is not directly deleterious, but its indirect effect in requiring a large and expensive solar collection area is serious. An alternative is the use of large focusing systems to permit generation of high pressure steam which can then be used at substantially higher engine efficiency, but there are optical losses in the reflector system as well as high investment requirements in the more complicated apparatus.

Both of these methods of generating electricity from solar energy are being studied. But the cost of very large reflectors and the complexity of their operation relegate the concentrating systems primarily to the field of small power units of only a few kilowatts capacity. Larger power plants would probably have to be composed of multiples of smaller units. At an optimistic investment requirement of $5 per square foot of reflector surface, and assuming this figure would cover all other auxiliary costs, the 20 to 40 kilowatt-hours this surface might produce in a year in a sunny climate would have a "fuel" cost equivalent of over 1 cent per kilowatt-hour. In comparison with average fuel prices of 0.3 to 0.4 cent per kilowatt-hour generated in the United States, these figures show that large scale production of electricity from solar energy by means of focusing systems is in the distant future. Some development work is being done along these lines, but the principal efforts with focusing systems are now directed toward small scale power generation.

Some preliminary economic studies of the large scale generation of electric power by use of flat-plate collector designs as now known have shown that amortization of the required investment would cost several cents per kilowatt-hour generated. Most of these evaluations have not included any form of energy storage, so power would be available from such during sunny periods. Hence the value of this power would be equivalent only to the cost of fuel saved by a companion plant of full capacity. Estimates have shown that U.S. fuel prices would have to rise two- or three-fold before electricity generated from solar energy by such means could be significant in this country.

Another approach to the problem has recently made economical large scale solar power appear more promising. Research in Israel and elsewhere has shown that artificial water ponds of moderate depth, when protected from excessive heat loss by use of transparent covers or by reducing thermal convection in the water, can be solar heated to temperatures adequate for power generation from the resulting low pressure vapor. Although efficiencies at these temperatures are low, a very cheap "boiler" such as a simple pond of water, with minimum added investment for heat loss reduction, might permit power generation at less than a cent per kilowatt-hour. Such a plant should be able to supply firm power because of the inherent thermal storage feature. Research on materials of construction, techniques of operation, and other factors must go considerably further before the potential of this concept can be appraised.

B. Small scale power generation

In this discussion "small scale" may be considered a power capacity range of a few kilowatts. In use, these power generators would usually be operated by the consumer, no transmission of electricity being involved. This immediately suggests the use of such systems in areas where electricity is either not available or may be supplied by transmission line only at very high expense.

1. *Photoelectric generators.*—One of the most spectacular applications of solar energy in recent years is the so-called solar battery. This usually consists of a metallic silicon wafer of high purity, with traces of other elements to give it the property of a semiconductor. When exposed to solar radiation, the best of these cells can convert up to 15 percent of the radiant energy to electricity, and even the average cell is now capable of nearly 10 percent efficiency.

A number of small scale uses of solar cells have already been introduced, and others are being developed. Although silicon cells have comparatively high efficiency, they are costly per unit of electric capacity. One kilowatt of daytime capacity requires about 100 square feet of silicon cells at an investment of over $100,000. The applicability of this device appears now to be limited to small electrical loads, such as in communications equipment. Use in radio, telephone, signaling, and other devices is growing. The most spectacular demonstrations have been the powering of radio transmitter equipment on satellites and space probes. Where the cost of this power supply is such a minor part of the entire investment, this method of furnishing small amounts of electric power appears very attractive.

Much research is being directed toward increasing cell efficiency and reducing costs. Although improvements can be expected, tremendous differences are not foreseen. Therefore, solar power from silicon cells is likely to remain expensive and of special applicability. Other photoelectric materials may come into use, and possibly at lower cost, so the potential of this development might increase.

2. *Thermoelectric generation.*—Dramatic progress has been made in the last 3 years in the conversion of heat to electricity by means of the thermoelectric principle, i.e., the supply of heat to a junction of two different conductors, thereby causing a current to flow through a complete circuit. Although this principle has long been known, only recently have new materials been tried and conversion efficiently increased from the fraction of 1 percent formerly achieved. Although conventional heat sources have been involved in most of the development work, there is every reason to expect that concentrated solar energy will be useful in supplying thermoelectricity in remote areas. At temperatures near 1,000° F. the efficiency of the new thermoelectric systems can be as high as 10 percent. When fully developed, the thermoelectric elements are expected to be comparatively cheap per unit of electrical capacity. With no moving parts and practically unlimited life, these units, in combination with cheap plastic reflectors, may be of exceptional importance in supplying up to a few kilowatts for pumping water, lighting rural buildings, and other uses. Discussion of this potential in the underdeveloped countries is presented by Dr. Duffie.

3. *Thermionic power generators.*—Recent experiments have indicated that heat can be converted to electricity in an electronic tube where very high temperature heating of a cathode causes a flow of electricity through the tube to an anode and an external circuit. The placement of the cathode at the focus of a precise solar reflector can accomplish the purpose. Efficiencies in the 20 percent range may be possible in such a system. Although the 4,000°–5,000° F. temperatures will require high precision in the reflector, the potential nevertheless is interesting enough to justify further development.

4. *The hot air engine.*—Although the hot air engine (sometimes known as Stirling engine) was discarded generations ago and replaced by the reciprocating steam engine and later by the steam turbine, it might again become useful in modified form. Recent developments abroad have led to major reductions in the bulkiness of the equipment and may permit practical operation in a solar concentrating system. By use of air rather than steam and alternately heating and cooling it by movement back and forth within the engine itself, pressure rises and falls in the cylinder, thereby moving a piston. Comparatively high efficiency can be achieved in an engine of this type if temperatures are adequate. Experiments are proceeding in several laboratories not only with improvements in the engine itself but with combinations of focusing solar reflectors and small hot air engines. A reflector of about 100 square feet could possibly develop 1 horsepower, and with further improvement perhaps as much as a kilowatt.

C. Auxiliary power in space

The only available energy in space in any substantial quantity is solar energy. In the general vicinity of the earth's orbit its intensity is nearly half again as large as on earth. The powering of communications equipment and instruments on satellites and other space vehicles by means of solar energy has already been successfully demonstrated with the silicon cell. With the establishment of more permanent and perhaps manned space stations or planetary-based outposts, power requirements will grow substantially. The continuous solar energy supply (except when in the shadow of celestial bodies), the lack of air to impede or destroy lightweight structures, the lack of any radioactivity or other hazardous byproducts, make solar energy a strong contender for filling such requirements. Competitors of the silicon cell are thermoelectric and thermionic converters and even reasonably conventional vapor or hot air engines all in conjunction with focusing solar reflectors. With an average 10 percent overall conversion efficiency, solar concentration areas of 60 to 70 square feet per kilowatt of capacity would be required. The transport into space of large solar reflectors in a folded or deflated form, inside a compact and lightweight package in a rocket appears entirely feasible. These systems for supplying auxiliary power in space are receiving much consideration. It can be expected that further spectacular developments will soon be forthcoming in these new and challenging areas.

D. Long range research in solar power

A tantalizing goal in the field of solar energy conversion has long been the artificial duplication or improvement on natural photosynthesis. By means of processes not fully understood, living plants can convert solar energy to chemicals, store them, and make them available for subsequent use. With the exception of our nuclear fuels and hydroelectric power, all of our energy economy is based on the occurrence of this process in past geologic ages of the earth. The efficiency of converting solar energy to chemical energy by natural processes is extremely low, probably averaging less than 1 percent. Moreover, the products of the conversion usually have inconvenient bulk, e.g., leaves, wood, etc.

If a chemical reaction or combination of reactions could be found whereby solar energy could be efficiently captured in a way that would cause the reaction to proceed, thereby storing energy for an indefinite length of time, then the process could be reversed and the energy released at will. Photochemists have been working in this field for years, and several interesting possibilities have developed. But the energy conversion efficiencies have so far been extremely low, in fact even below those of natural processes. Only the ultraviolet portion, or highest energy part of the solar spectrum, has been successfully utilized in such reactions as water decomposition into hydrogen and oxygen.

Another, perhaps even more interesting prospect is for sunlight to cause a chemical reaction which on reversal will yield electricity directly. A photogalvanic process of this sort would be carried out in an electrolytic cell, where a reaction would go one way when exposed to sunlight, and when darkened, go in the other direction to yield an electric current. Only the most limited progress has been made in this field, and much further work is required before its potential can be appraised.

These fundamental research studies may prove to be of great practical value if some fortunate factors materialize. Not only the conversion of solar eneregy to electricity but also its intermediate storage would then be accomplished in one overall process. Since the chemicals would be used over and over again, their cost would not be significant in the energy production economics. It must be emphasized that these are speculative and long-range potentials, with no firm assurance of realization. But in comparison with thermonuclear energy, described elsewhere in these hearings, this potential cannot be considered less remote nor less important.

III. SPECIAL APPLICATIONS OF SOLAR ENERGY

Several present and potential uses of solar energy involve neither direct heat production nor power generation. Instead, some mineral or agricultural product is involved.

A. Sea water demineralization

One of the promising processes for low cost sea water demineralization to produce domestic and industrial fresh water supplies involves very simple, direct utilization of solar energy in the evaporation of sea water from shallow artificial basins. If these basins are provided with sloping transparent covers, the water vapor condenses on their undersides and runs into edge troughs and to storage. Pilot plant experiments are in progress in Florida under the auspices of the Office of Saline Water of the U. S. Department of the Interior. Several solar distiller designs, including asphalt basins, glass covers, and all-plastic units, are being tested. Capable of producing nearly 5,000 gallons per sunny day per acre of pond area, these systems involve costs almost exclusively the amortization of the original investment. Much effort is being devoted to the development of designs of exceptional economy in materials and fabrication.

In arid regions where coastal deserts are common, ample solar energy is usually available and fuels may be costly. Solar distillation of sea water should become of considerable importance in the years ahead, as water demand and price rise sufficiently to justify the greater cost of this demineralized supply. Development of new and simpler designs and processes are steadily leading toward practical utilization of this system for augmenting water supplies.

B. Salt production

Salt production by solar evaporation of sea water and salt lakes is a practical solar energy application of long standing. It is mentioned here primarily as an illustration of a mature solar energy application, in contrast with most of the other uses which are in various stages of development. The world's solar salt production is in the millions of tons per year, and various other chemicals such as soda ash, borax, and potash are obtained as byproducts. Most of the U.S. production is on the California coast and the Great Salt Lake of Utah.

C. Algae production

A considerably different type of solar energy application is the growth of algae, simple, waterborne plants, for animal and possibly human foods. In the presence of carbon dioxide and water, algae can convert solar energy to carbohydrates and proteins at efficiencies considerably higher than can ordinary land crops. Although the economics of the process are currently not as attractive as most land-based agriculture, any future shortage of land for crop production could be offset by this alternate food source. Development of this process is now concentrated in Japan.

D. Solar drying

Although the sun has been used for centuries in the drying of crops, fish and meats, there are potentials for more efficient and general use of solar energy for drying purposes, particularly when augmented by modern equipment. For preservation of perishables, such as livestock feed, fruits and vegetables, meats, and the like, for product treatment as in coffee drying and dehydrated food production, and for raw material preparation as in coal and oil shale drying, solar energy use may become increasingly important. Both for certain unique advantages of cleanliness, and also for the fuel savings possible, solar drying is of interest. A recent study of the solar drying of water-saturated Brazilian oil shale indicates, for example, that the overall economics of the entire oil production enterprise might be dependent on the feasibility of this preliminary drying step.

IV. CONCLUSION

In a nation as rich in energy resources as the United States, the need for applying solar energy to any of our conventional requirements appears quite remote. But the same might be said of atomic energy. But there are two or three aspects of this question which make this first view not entirely valid. First, if the cost of these alternate supplies can be reduced to the point where they are better than competitive with existing sources, there is every reason to develop and utilize them. Secondly, departing from the limitations of the present and looking to the welfare of future generations, conservation of high value resources for other uses by development of these alternate supplies may well be a desirable current policy. The hydrocarbons, for example, must serve as raw materials for many types of products—chemicals, clothing, building materials, and a host of other products. And finally, in the world view, solar energy is actually needed now in many places on earth. The economic potential and the technical background for developing these sources does not exist in most of those regions, so countries having such potential, and particularly the United States, must be looked to for these developments. Solar energy falls clearly into this category; its technology would be a most valuable "export." My distinguished colleague will emphasize the value of this policy in his testimony.

In closing, let me state my opinion that in proportion to the very modest expenditures, development of solar energy for man's use has shown remarkable progress. But progress could be far more rapid, were funds and facilities available. In photochemical research, for example, the long-sought "breakthrough" might come much sooner. With technical and economic prospects no less attractive than many of the well-supported atomic power studies, but with perhaps only one-thousandth the funds, solar energy research and development deserves substantially greater support.

Representative PRICE. The final witness this morning is Dr. John A. Duffie, of the University of Wisconsin, who will discuss solar energy applications in less developed nations.

STATEMENT OF JOHN A. DUFFIE,[1] DIRECTOR, SOLAR ENERGY LABORATORY, ENGINEERING EXPERIMENT STATION, UNIVERSITY OF WISCONSIN

Dr. DUFFIE. Mr. Chairman, I would like to submit to you three photographs. These photographs are of recent origin and I received the prints only this morning, so I apologize for not having them in the form that would be most advantageous. But they are quite recent.

Representative PRICE. Thank you, Doctor. We will accept them for the record, where you refer to them.

Representative BATES. Are you going to refer to these photographs, Dr. Duffie?

Dr. DUFFIE. Yes, sir; I will.

Dr. Smyth on Tuesday noted a correlation between energy consumption and standard of living. I shall address myself to the possibilities of an energy supply for those of the world population who now consume very little energy and whose standard of living is correspondingly low.

Dr. Löf's statement has been in contrast to testimony before the committee on previous days of these hearings. I should like to draw you a picture in even sharper contrast to the previous proceedings.

The contrasts are in the complexities of the operations and processes of supplying and using the energy, the economic status of the people who may make use of the energy supplies, and in the extent of effort devoted to the solution of problems relating to these supplies.

The contrasts are sharp, but I think that these two energy sources, atomic and solar, should complement each other in these less developed countries which are now hindered by their lack of usable energy supplies.

Dr. George Löf, in his testimony to the committee immediately preceding this statement, has described the principal solar energy applications of interest in domestic and industrial uses, and has presented information on the intensity and availability of solar radiation.

The following remarks will amplify Dr. Löf's comments, with particular regard to the unique aspects of solar energy applications in less developed areas, to research in this field, and to the potential scope of applications in these areas.

Most solar energy research has been done in industrialized nations where some funds and manpower have been available for it.

As these researches have developed, the potential of solar energy applications in less developed nations has become evident.

It is appropriate that the United States should be interested in these applications in less developed areas. Successfully made, they can con-

[1] Born White Plains, N.Y., 1925. Attended Rensselaer Polytechnic Institute; degrees, B. Ch. E., 1945, M. Ch. E., 1948; also instructor in chemical engineering, 1946–49. Attended University of Wisconsin, 1949–51; degree, Ph. D., 1951. Employed as research engineer, Electrochemicals Department, DuPont Co., Niagara Falls. Served as scientific liaison officer, Office of Naval Research, Chicago, 1952–53. Employed as project associate, University of Wisconsin, solar energy research program in 1954. Editor (with Farrington Daniels) and contributing author, "Solar Energy Research" and author of 10 publications in the fields of drying and solar energy utilization. Now director of the solar energy laboratory, associate professor and assistant director, Engineering Experiment Station, University of Wisconsin.

stitute direct evidence of the interest of this Nation in the well-being of people of other nations.

They can also meet needs whch now appear to be outside of the scope of atomic and other energy resources.

Many nations of the arid zones of the world are poor in conventional energy resources, and have high solar radiation. The use of this renewing natural resource has the potential of significant contribution to the energy economy of these nations and the potential of raising the standard of living of the peoples of these nations.

Efforts to make the resource useful are inherently peaceful and humanitarian in nature, and are aimed at improving human well-being.

This discussion is directed at applications in these fuel-poor, sun-rich nations.

The development and application of solar devices in nonindustrialized areas as a potential means of raising the standard of living of peoples of these areas has been the subject of a 5-year research program at the University of Wisconsin.

As part of this research, a study is being made of the social utility and acceptability of solar devices in villages in Mexico, in a cooperative program between the anthropology and engineering departments.

These are photographs (pp. 369–370) taken in Mexico early this month, and show a solar cooker in use by a family in a village in the state of Coahuila, in the northern part of the country.

The people pictured are in many ways typical of millions of the world's population who are potential users of solar energy. They live in a village of adobe houses in a climate predominantly sunny. They are for the most part agricultural workers who earn, when work in available, less than a dollar a day. They ordinarily cook with wood or oil, and in many families a substantial part of their income is spent for fuel for cooking.

Most of the 16 families having cookers find that their use results in saving of a large portion of the usual expenditure for fuel.

While these studies have been limited in their extent and have concerned small numbers of people in this and another village, the tentative conclusion has been drawn that this device is technically and socially suitable in these villages.

Two principal questions remain. One concerns the ability of these people to make the investment necessary to take advantage of the economic and other gains to be had by use of the cooker.

The other concerns the generalization of these limited experiments to other cultural situations, which may be less favorable than those in the villages under study.

Representative PRICE. As a matter of curiosity, how effective are these solar cookers that you show in the picture?

Dr. DUFFIE. Properly used on a good day the output of these units roughly is equivalent to medium heat on an electric hot plate.

Let us say 600 or 700 watts.

Representative PRICE. If you ran into a cloudy day, you might go without dinner?

Dr. DUFFIE. That is right.

These are not substitutes for conventional cooking methods.

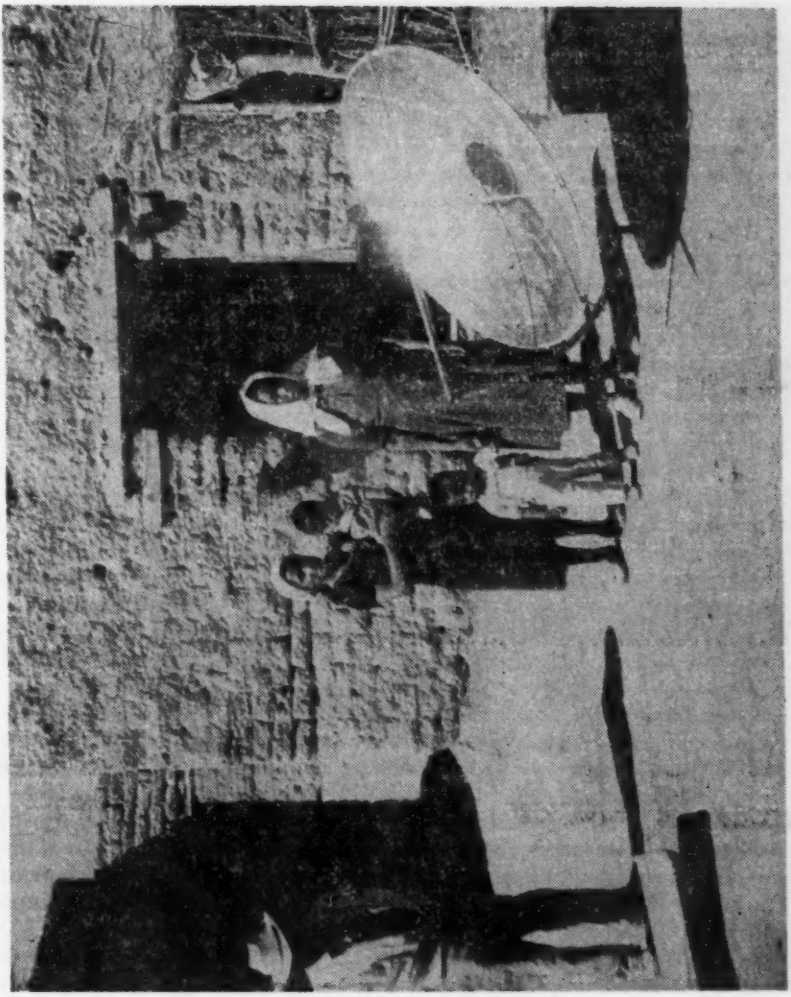

FIGURE 1A.—Solar cooker in use in Mexico.

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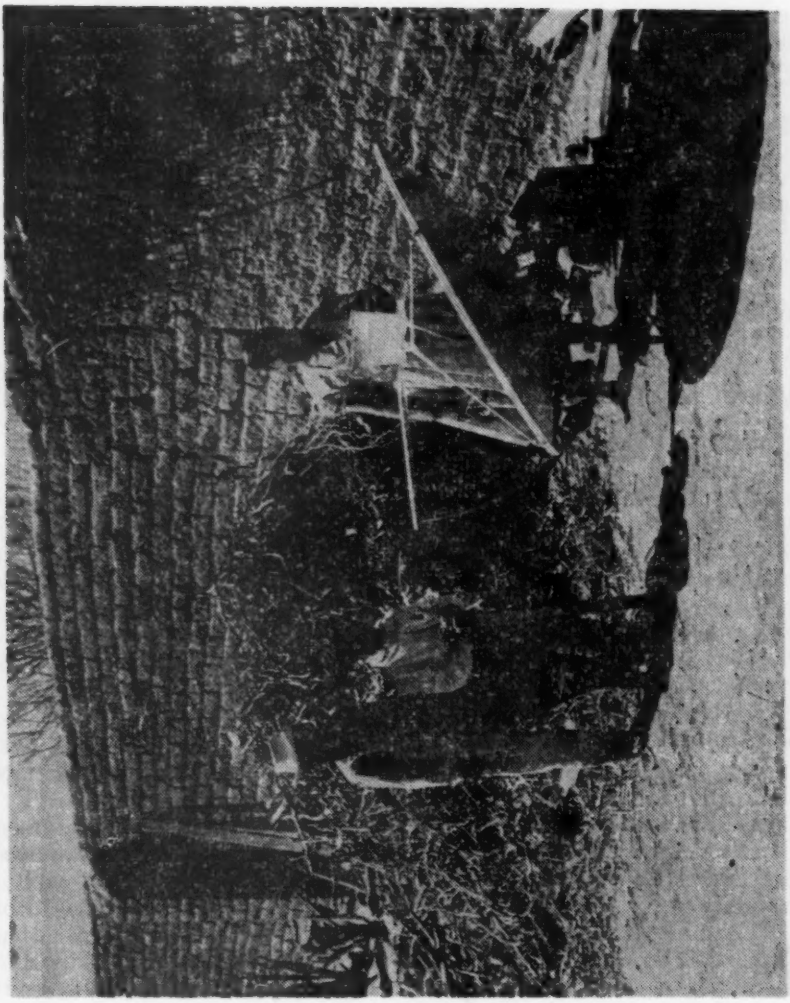

FIGURE 1B.—Solar cooker in use in Mexico.

Representative HOSMER. Is this the one developed at Wisconsin University?

Dr. DUFFIE. That is right.

Representative HOSMER. There is a lady scientist at Columbia who has been working on some kind of cooker that costs less than $10.

Dr. DUFFIE. That is correct.

Representative HOSMER. Have you familiarity with that thing?

Dr. DUFFIE. Yes; I have.

Representative HOSMER. How does that compare with what you have in the picture?

Dr. DUFFIE. It is a different kind of a device adapted to different kind of cooking operations. I don't think I am prepared at this stage to make a comparison of the relative costs of these two items. I don't think they are vastly different.

Representative PRICE. What does your research tend to do for the cooker to make it of permanent value rather than spotty value? Have you a storage system?

Dr. DUFFIE. It appears at the present time as though the addition of a storage unit would increase the cost of the cooker to such an extent that it would not be economically feasible for this kind of a consumer.

Representative HOSMER. Are you familiar with the arguments that this lady—I have forgotten her name—do you recall?

Dr. DUFFIE. Dr. Telkes.

Representative HOSMER. That she had for us on India. I think it is applicable in answer to Mr. Price's question.

Dr. DUFFIE. The solar oven which was developed at New York University does have some heat storage capacity and can be used at least in periods of intermittent sunshine. It has a limited heat storage capacity.

Representative HOSMER. What I had in mind was the fact that over in the vast continent of India they use considerable animal manure for cooking fuel.

Dr. DUFFIE. That is correct.

Representative HOSMER. If on sunny days you could eliminate using it for that purpose and utilize it for fertilizer, you would have a great effect upon the culture and economic conditions of the country?

Dr. DUFFIE. This is one of the very good justifications for being interested in this application. There are others, for example, conservation of forestlands, aside from the economic gain that people have to make on a very personal and family level by use of this kind of equipment.

Characteristics of applications in less developed areas:

Technological development, to be ultimately useful to man, must meet in varying degrees the tests of technical practicability, economic feasibility, and human acceptability.

These problems exist in less developed nations, and for solar energy applications the nature of the energy resource and the status of the potential users impose unique economic and sociological problems which must be solved before widespread use of the resource can be made.

The best prospects for future applications in less developed areas are on a small scale, i.e., on a household, farm, small industrial, or small community scale.

The applications of solar energy will probably be in the form of a multiplicity of small installations, rather than in fewer large central power stations.

Thus the potential of solar energy in these areas appears to be in rural areas of low population density and low per capita energy consumption.

Several factors lead to this conclusion. The low intensity of solar radiation dictates large areas of energy collecting surfaces for large outputs, and the technical problems of scale up of solar collectors are difficult.

The costs of energy distribution from central power stations to areas of low population density, problems of collector maintenance cost, and the competition of energy from conventional sources, when produced in large plants, all indicate the most probable applications to be on a small scale.

The prospects for small-scale applications, with relatively simple equipment purchased by small investment per unit, without the complication of hazards of atomic energy installations, thus are in contrast with those of atomic energy.

Atomic energy in large-scale plants can meet needs of more heavily populated urban areas. Solar energy will find its applicability in smaller installations. A reasonable energy source picture of a decade or two hence should show these resources complementing one another in meeting the total energy needs.

Two large-scale applications of solar energy are now being made and are of historical significance.

The production of salt by solar evaporation of water from brines in open ponds has been an industrial operation for centuries, and is practiced in many arid parts of the world, including Israel, Mexico, Colombia, and the United States.

Solar drying of fruits and vegetables is carried out on both small and large scales to process and preserve food crops.

Two other historical applications were a 50-horsepower solar irrigation pump operated in Egypt in 1913, and a solar still of more than an acre in extent built in Chile in 1872 to provide about 4,000 gallons per day of potable water for humans and animals engaged in a mining operation.

Thus, some of these operations of which we speak are not very new.

Technological problems in solar applications in less developed nations are essentially the same as those described by Dr. Löf.

Additional considerations important for applications in these areas include ease of operation and maintenance of equipment by unskilled personnel, the effects of local meteorological conditions on equipment design operation, long time reliability, and transportability of equipment to the areas of its use.

The progress of research and development on applications is dependent on basic research in sciences and engineering, and there are many gaps in our basic knowledge of solar conversion processes that are yet to be filled.

I think Dr. Egli has pictured researches that are applicable to solar applications in either developed or less developed areas.

For example, solid state physics research has led to the development of silicon solar cells and greatly improved thermoelectric power conversion devices.

Researches in photochemical and photoelectric processes have long-range potential for opening new possibilities for solar applications.

The progress of basic researches will certainly greatly influence the applicability of solar energy in both industrialized and less developed areas.

Economic problems: Solar energy applications are characterized by high first costs, and low operating and maintenance costs. It is unfortunately true that peoples of nations having the most to gain from the applications are also those least able to afford investment in equipment, and widespread application in less developed areas may be contingent on development of means for financing the purchase of equipment.

The high first cost has also determined to a substantial degree the direction of engineering research on applications.

The prime objectives of much of this research include reduction of cost per unit of output of the solar process through reduction of the first cost, improved efficiency—output per unit area of the device—and low maintenance and operating cost.

On an economic basis alone, the widespread use of solar energy cannot be expected to develop until energy from the sun can be delivered and used more economically than can energy from other sources.

Some of the factors determining costs of solar energy applications, and the competitive position of solar heaters, coolers, and powerplants, have been reported to a congressional committee.[1]

It was concluded that solar energy has the potential of competing with energy from conventional sources, particularly where the costs of delivered energy from the conventional sources are high.

This situation exists in many less developed nations, where the lack of inexpensive energy has been a factor retarding economic development.

Thus the provision of a more economical energy source, based on a renewing natural resource, can contribute to the development of these areas.

Sociological problems: The human problems of acceptance of the products of new technology are particularly important in the small scale applications to which solar energy appears best suited.

Potential users must be willing to adapt their habits to the requirements of operation of the new device, and the device itself must meet a felt need of the user, or a need must be created, before it will be readily acceptable.

It may be necessary to educate the users in the benefits of and requirements for use of a new device, and the prediction of the utility and economic worth of a small scale device is an uncertain task, having many ramifications beyond the purely technical.

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[1] Energy Resources and Technology, hearings before the Subcommittee on Automation and Energy Resources of the Joint Economic Committee, Congress of the United States, Oct. 16, 1959, p. 305.

The social structure of the community, living habits, religious beliefs, contact with other products of technology, social and economic status of the users, and other factors within the range of interest of the cultural anthropologists may all be of importance in determining the utility of the products of engineering research and development.

Progress in the introduction of solar applications, particularly on a household or family scale and in the early stages of the developments, will in many areas be substantially dependent on anthropological research. Studies now in progress on the social use of solar cookers in Mexico have shown the importance of education of the users, their day-to-day living and eating habits, the availability and costs of energy for cooking from other sources, and intangible factors such as impressions of different taste of solar cooked food.

Present research efforts: A brief description of the University of Wisconsin solar energy research program is presented; in its technical aspects this program is similar in nature to studies in other laboratories, and the program now includes a substantial degree of study of cultural problems associated with solar energy applications in less developed areas.

Following this description, a brief survey indicates activities of some other groups particularly concerned with applications in less developed nations.

The Wisconsin program has as one of its major aims the study of the utilization of solar energy with particular reference to the less developed areas of the world. With this general aim, engineering problems under consideration include basic studies of solar collectors and solar radiation.

The development of equipment for several applications has been undertaken, these including solar cookers, water heaters, power systems for conversion of solar to mechanical or electrical energy, space coolers, and refrigerators.

With equipment provided by the engineering laboratories, the study of the social utility of the developments is undertaken by anthropologists. These studies are conducted in the field by personnel who live with the villagers and work with the solar equipment to instruct the users and observe the kinds and extent of use of the equipment.

Solar cookers and water heaters are now in the field test stage, and a food cooler is read for first field trials.

A recent model of the cooker was previously shown in figure 1. Small water heaters for domestic use are in use on the Colorado River Indian Reservation; they will produce about 8 gallons of hot water by midafternoon of a clear day, and do not require any attention other than filling of the tank in the morning.

The solar food cooler is an intermittent heat operated system simple in construction, but requiring manual manipulation in its operation; it uses the solar cooker as the heating means, and will keep a 2 cubic foot box cool enough for food preservation.

The villages where the cooler is to be studied do not now have refrigeration other than that provided by occasional purchases of ice.

Thus, the introduction would provide the advantage of food storage and attendant improved food sanitation, benefits not now available.

These concepts are new to most of the people of these areas, the benefits are not obvious, and the anthropologists will face a new set of problems in the introduction of the cooler.

Early findings by the anthropologists in their field studies on the cooker have led to engineering modifications and improvements, such as increased heat delivery capacity to make cooking on the solar unit more comparable to that on the wood fires usually used.

Acceptance of the cooker as a useful device for food preparation, milk boiling, water heating, and for other heating purposes, is very good in these villages.

Additional uses are being found, including the boiling of drinking water and the heating of flatirons.

The potential costs of the cooker in these areas and the ability of the villagers to purchase them are subjects of continuing study. Present estimates indicate cooker costs of the order of $10.

The application of solar energy on a village or small industrial or commercial scale appears to present fewer sociological problems than does application on a household scale.

Two other applications are now being studied for one of the Mexican villages where cookers are in use. A solar boiler for a family scale wool dyeing and weaving business appears to be a feasible system.

The village water supply is now obtained by laborious hand lifting of water from a well more than 300 feet deep by the device shown in figure 2. A fractional horsepower solar pump could relieve the necessity for a difficult manual operation. Systems under study for this pumping operation include a solar thermoelectric generator, with a small electric pump lowered into the well.

Heat would be supplied to the hot junctions of the generator, and its operation would be essentially as described by Dr. Egli.

Also, the advantages to be gained by the use of this direct conversion device are substantially as outlined by him, i.e., the advantages of simplicity and lack of moving parts.

The second type of system being studied for this application is based on the use of a reflector to supply solar heat to a closed cycle air engine. The engine is similar in operation to those built by Mr. Ericsson, the inventor of the *Monitor*, to whom Mr. Hosmer referred.

But the engines that we are studying are vastly improved by recent research and development.

Other laboratories engaged in research directed at applications in less developed nations include:

New York University: Studies of a solar oven and solar stills;

The National Physical Laboratory of Israel: Studies of cooling and refrigeration, small power units, steam boilers, et cetera;

The Heliotechnical Laboratory of Moscow: Studies of steam generation, refrigeration, solar cookers, and thermoelectric converters; and

The National Physical Laboratory of India: Studies of solar cookers, small power units, sugar cane and palm juice concentrators, and other applications to small scale industries.

There are other smaller research groups, and a comprehensive listing of research activities is available.[2]

Much of the solar energy research directly applicable to less developed areas is done in industrialized countries where funds have been available to support the work.

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[2] Applied Solar Energy Research, the Association for Applied Solar Energy, Phoenix, 1959.

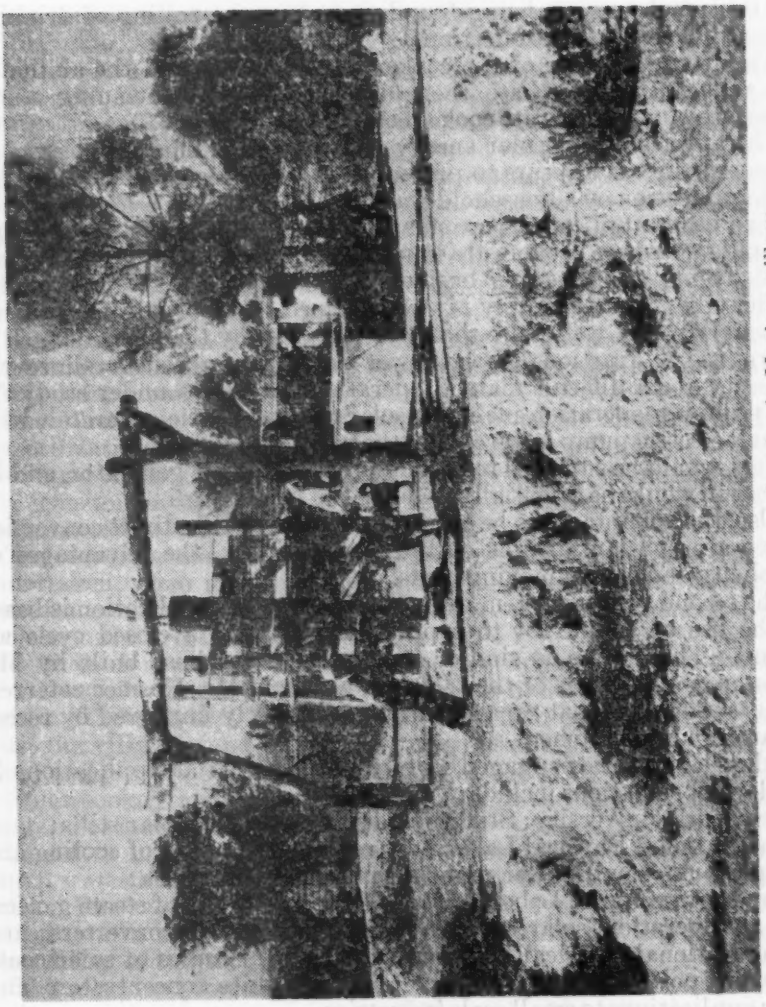

FIGURE 2.—Hand pumping of water in Mexican village.

In this country, the Wisconsin program has been supported generously by the Rockefeller Foundation, and the Ford Foundation has supported the research at New York University.

Studies abroad have more often been Government financed.

Further research and applications: The total number of scientists and engineers engaged in all phases of solar energy applications research through the world—other than for space vehicle application—is probably in the range of 100 or 200. This modest level of effort, most of which is directed toward thermal applications, together with the difficult economic and sociological problems, has resulted in limited progress on applications.

However, the technology now developed can be expected in the next decade to contribute in a substantial manner to the energy economy of less developed areas of good radiation where conventional energy resources are expensive or unavailable.

The present state of development indicates that the most significant possibilities for application are power units of a few horsepower, solar coolers to improve health and working conditions in hot climates, solar cookers, solar stills and water heaters.

The effects of successful widespread applications of solar energy by less developed nations will be of real benefit to their peoples in meeting physical needs.

These developments are free of any connotations of other than peaceful applications, and should have a favorable influence in improving relationships between the people making the applications and those who have contributed to the development of the necessary technology.

Continued and expanded research efforts, on both the fundamental and applied aspects of solar energy utilization, and concerning the technological, economic and sociological areas, is necessary and vital to assure that this energy resource will be utilized to substantial extent to improve the lot of peoples of less developed nations.

Solar energy technology as we now know it is a relatively new field and its continued development can assure this Nation of a long-range energy supply and result in improved living conditions for millions of the world's population.

Representative PRICE. Thank you very much, Dr. Duffie.

Mr. Hosmer, do you have any questions?

Representative HOSMER. Dr. Duffie, you are director of this program. It is a 5-year program. When did it start?

Dr. DUFFIE. The program is 5 years old now.

Representative HOSMER. It is ending, then; is it not?

Dr. DUFFIE. No; it will continue with Rockefeller support for 2 more years. Where it goes beyond there depends on what kind of financing and personnel arrangements can be made.

Representative HOSMER. Is it a matter of public record the amount of support Rockefeller Foundation has given you?

Dr. DUFFIE. Yes; the total support for the 7-year program is half a million dollars. It is roughly $70,000 to $80,000 a year.

Representative HOSMER. How did this program develop its relationship with the Rockfeller Foundation?

Dr. DUFFIE. This question is tied in with another one which might well be asked, "Why is solar energy research done in Wisconsin where there is little solar energy, at least for a good share of this winter?"

The answer to both of these questions lies in the people involved. The program came to Wisconsin and received support from the Rockefeller Foundation largely because of the eminence of Prof. Farrington Daniels, who is, incidentally, vice president of the National Academy of Sciences, and who is now on a tour of India, Pakistan, and the Near East.

Representative HOSMER. What is Farrington Daniels' field?

Dr. DUFFIE. He is a chemist.

Representative HOSMER. How did the anthropologists get mixed up in it?

Dr. DUFFIE. One of the first developments of the engineering program was an early modification of the solar cooker pictured in figure 1. The Rockefeller Foundation had expressed a wish, when they first supported this program, that we might carry these researches to a field test stage.

Accordingly, several of us who are engineers and not anthropologists made efforts to try to assess the utility of this particular device in studies carried out in Mexico. We found that we were not good anthropologists; that training in areas of human relations and so on was necessary in order to properly evaluate the utility of the cooker, and we called in our anthropologits who responded in a most gratifying fashion.

Representative HOSMER. Aside from all that, do you think you would have gotten your money from the Rockefeller Foundation if you had not dragged in the anthropologists?

Dr. DUFFIE. We did get commitment for all of the half million dollars before the anthropologists were concerned with the program.

Representative HOSMER. This wish that was expressed regarding the anthropologists, when was it expressed in relation to the commencement of the program?

Dr. DUFFIE. I think it was expressed at the time of the first grant of funds. The funds came in two grants. The wish was put in terms of a desire that we should carry any developments to a field test stage.

Representative HOSMER. Did you, as a chemist, have any interest in the sociological aspects or were you delving into the physical in your own mind when you started this thing?

Dr. DUFFIE. I am an engineer. We were primarily concerned and I am still primarily concerned with the physical aspects of the problems.

Representative HOSMER. The reason I ask you these questions is that I am concerned that this large-scale foundation money oftentimes instead of going directly, pinpointed to a particular field of physical research, gets itself wound up in sociological factors which I personally do not approve of.

However, I do not own and control the funds as much as I would like to. I wanted to bring it out on the record.

Dr. DUFFIE. May I make one further comment in that regard, Mr. Hosmer?

Representative HOSMER. Yes.

Dr. DUFFIE. I should further amplify our relationship with the Rockefeller Foundation by stating that the anthropological portion of our program is actually supported by a separate grant of a much more modest level of, I think about $15,000 a year, which has just recently

been received. So the half million dollars is primarily expended in the research on the physical aspects of the problems and these small additional funds are available for this other aspect of the overall research program.

Representative HOSMER. Can you give us a reasonable evaluation whether you are more affected by the anthropologists or the anthropologists more affected by you in carrying out the program jointly?

Dr. DUFFIE. They have provided a useful guide for us in developments on the basis of information that they have obtained. We feel now for the first time that we can say we have a serviceable solar cooker, for example. We were never sure of this before.

Representative HOSMER. I think the question of whether or not the Mexicans can cook on it or not is the real test.

Dr. DUFFIE. That is right.

Representative HOSMER. What did the anthropologists have to do in deciding that, that you could not do?

Dr. DUFFIE. The problems are twofold. First, in designing the experiments to determine whether these people will or will not use them. For example, it is not at all obvious as to whom you should go, or how you approach people. You go into a village where you have a limited number of items of equipment. Who do you select in this village as being typical of the economic status of the village? Are they liked or disliked, or looked up to or looked down on by the others?

We found literally that the results we got as engineers in our field test program were very sensitive to these factors, of which we had very little appreciation.

The second problem coming out of this is one of obtaining information. Our technique involved occasional trips to Mexico to try to find these people who had these units. This is not an effective way of obtaining information.

There is a great tendency for people to say what they think you want to hear and this sort of thing. Somebody who is on the scene is much more skilled at discerning the true reaction of the people to this or any other new innovation.

Representative PRICE. Doctor, as a scientist, are you just interested in research for the purpose of producing a gadget, or are you interested in research for the purpose of producing something for the benefit of mankind?

Dr. DUFFIE. I think the latter case is true.

Representative PRICE. In other words, you could not divorce your interest to produce something new in the world without thinking of the good that will be generated in the social order of the world?

Dr. DUFFIE. That is correct. We are engineers, perhaps, as contrasted to what might be called pure scientists. We are interested in the applications.

Representative PRICE. You would not want to feel you were wasting your time on something that did not improve the world in some way.

Dr. DUFFIE. There are many reasons for doing this research.

Representative PRICE. You are not in the military field. You are in the area of research in a field that you hope will someday benefit mankind.

Dr. DUFFIE. I hope that these activities will benefit mankind.

Representative HOSMER. Do you seek knowledge for the sake of knowledge itself or do you seek knowledge because somebody else tells you that somebody can get some use out of it, maybe?

Dr. DUFFIE. As an engineer, I think the latter case is more true.

Representative HOSMER. As a scientist you would not give the same answer, would you?

Dr. DUFFIE. We find that the problems of development of these devices or in the development of these devices there are very challenging engineering problems. The one you see before you in the photograph is a very simple gadget. It is not typical of the kind of research that we do in the laboratory for the most part. We have some very fundamental engineering studies on a modest scale which are in the long run applicable to these less-developed areas.

Representative PRICE. Doctor, I do not think we are putting you on trial, or your philosophy. On this note that you seek knowledge for knowledge's sake, but you also hope there will be some benefit of mankind, I think we will excuse you and thank you for a very fine paper.

Dr. DUFFIE. I thank you for your attention.

Representative PRICE. In closing this hearing on the frontiers in atomic energy research, we wish to thank all of the distinguished scientists and engineers who have participated.

This hearing has provided us with a great deal of valuable information relative to the subject this committee has legislative responsibility for. The vision provided to us by the witnesses who have appeared here is most gratifying and assuring.

We expect to publish the testimony obtained in the hearings in full as soon as possible. The followthrough on the important frontiers described here should be our concern. We assure you this committee will do everything possible to assure that our Nation continues to be preeminent in these important fields of science.

I think that we have covered the areas in which our national laboratories have been active in the past or are continuing research and we may expect greater effort and attention to these areas in the future from our national laboratories.

The committee will stand adjourned.

(Whereupon, at 12:25 p.m. Friday, March 25, 1960, the subcommittee adjourned, to reconvene at the call of the Chair.)

INDEX

Page

A. Peaceful uses of nuclear explosives (Plowshare program) ---- 9–82
- Athabaska tar sands (oil sands) ---- 9, 20, 30–34
- Blanca ---- 66
- Chariot ---- 59, 61, 75
- Excavation ---- 20, 56–63
- Gnome project ---- 36, 61, 64–65
- High explosives work ---- 62–63, 69–82
- Mining ---- 40–42, 70
- Moho ---- 22, 23
- Neptune ---- 59, 60, 75
- Oil shales ---- 42, 43
- Power ---- 63–65
- Rainier shot ---- 21–23, 26, 42
- Tamalpais event ---- 26
- Water resource development ---- 65–67

B. Controlled thermonuclear reactions (Sherwood program) ---- 83–155
- Alice ---- 110, 115, 116
- Astron ---- 88, 89, 102–105
- Baby Alice ---- 111, 112
- Burnout ---- 139, 143
- Cryogenic coils ---- 111–113, 118
- DCX machine ---- 85, 88, 137–146
- Entropy trapping of plasmas ---- 96–98
- Foreign thermonuclear program ---- 84–86
- Mirror devices ---- 88, 89, 105–111, 114–118
- Model C stellarator ---- 91, 195, 151, 155
- OGRA ---- 85, 90, 91, 96, 152
- Pinch research ---- 99–101, 127
- Scylla shock-mirror devices ---- 88, 95, 97
- Stellarators ---- 88, 89, 146–149, 151, 153–155
- Table Top ---- 106, 107, 115
- Toy Top ---- 115
- U.S. thermonuclear program ---- 86, 87, 89–92
- ZETA ---- 85, 152

C. Advanced reactor applications ---- 158–234
- Breeder reactors ---- 186–189, 192, 194
- Characteristics of nuclear fuels ---- 158–165
- Chemical reactors ---- 170, 171, 173–176
- Chemonuclear reactors ---- 171, 173, 174
- Coal irradiation ---- 177–181
- High temperature reactors ---- 181, 182
- Isotope production ---- 182, 183
- Nitrogen fixation ---- 171, 172
- Plutonium utilization ---- 191
- Power production ---- 166, 168
- Roundtable discussion ---- 183–193

D. Space propulsion and power ---- 235–306
- Agena vehicle ---- 299
- Centaur vehicle ---- 236, 242, 243, 250
- Kiwi experiments ---- 240, 277
- Mercury project ---- 245
- Navaho vehicle ---- 243
- Nova vehicle ---- 238, 242, 248
- Orion project ---- 282–285

D. Space propulsion and power—Continued Page
Rover project 248, 275–281
Saturn vehicle 237, 242, 246, 248, 249
SNAP devices 293–306
Van Allen belt 286–288

E. Direct energy conversion and solar energy systems 307–380
Direct conversion 308, 324, 326, 328, 329
Fuel, cells 308, 320, 322, 324, 326
Plasma thermocouple 330–336
Sea water demineralization 355, 356, 358, 365
Solar applications:
Cells 308, 311, 373
Cooking 351, 362, 368–371, 374
Drying 356, 366, 372
Heat storage 348, 350, 360
Hot water heating 347
House heating and cooling 348, 349, 360, 361
Power 351–354, 362, 363, 365
Pumping 375
Thermoelectricity 313, 353, 364
Thermionic emission 316, 320, 353

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